THURSDAY, APRIL 21, 1881

SIR WILLIAM HERSCHEL[1]

III.

In the concluding chapter of his Memoir Prof. Holden presents a Review of the scientific labours of William Herschel designed to enable the general reader to follow the course of his work and discoveries, astronomical and physical, referring to the *Analyse de la Vie et des Travaux de Sir William Herschel*, published by Arago in 1842 for a more detailed and precise account suited to the professional astronomer; also to "A Subject-Index and a Synopsis of the Scientific Writings of Sir William Herschel," prepared by himself and Dr. Hastings, and forming one of the publications of the Smithsonian Institution.

Prof. Holden naturally commences his review with the improvements in optical instruments and apparatus effected by Herschel. Up to his time the principal aids to observation were the Newtonian telescopes of Short and the small achromatics of Dollond, the six-foot Newtonians of the former maker, aperture 9·4 inches, and the forty-six-inch achromatics of Dollond, aperture 3·6 inches, were much esteemed, and one of each class was in use at the Royal Observatory, Greenwich, in 1765. Herschel gives us some account of the progress of his manufacture of telescopes in his description of the forty-feet reflector presented to the Royal Society in 1795. When he resided at Bath, he tells us, he had long been acquainted with the theory of optics and mechanics, and wanted only that experience so essential in the practice of these sciences. This he gradually acquired by way of amusement in his leisure hours (we have seen that he was closely occupied in his profession as a teacher of music), and thus he made "several two-foot, five-foot, seven-foot, ten-foot, and twenty-foot Newtonian telescopes, besides others, of the Gregorian form of eight, twelve, and eighteen inches, and two, three, five, and ten feet focal length," in all, as already stated, he made not less than 200 seven-feet, 100 ten-feet, and about 80 twenty-feet mirrors, in addition to the Gregorian telescopes. The number of stands he invented for these instruments he states it would not be easy to assign. Proceeding further, as early as 1781 he had designed and commenced the construction of what he terms "a 30-feet aërial reflector," and invented and executed a stand for it; he cast the mirror, "which was moulded up so as to come out 36 inches in diameter," but "the composition of the metal being a little too brittle, it cracked in the cooling." It was cast a second time, but here the furnace gave way and the metal ran into the fire. These accidents and the discovery of Uranus, which introduced Herschel to the patronage of the king, put a temporary stop to the construction of a great telescope. In 1783 he finished "a very good twenty-feet reflector with a large aperture," and after two years observation with it, became so convinced of the advantages of such apertures, that he recurred to his previous intention of increasing them still further. Soon afterwards, by the representations of Sir Joseph Banks, president of the Royal Society, Herschel, as his sister relates, obtained "the promise that 2000*l.* would be granted for enabling him to make himself an instrument."

The forty-feet reflector, the *chef d'œuvre* of Herschel's optical and mechanical efforts, was commenced about the latter end of 1785, and, as Prof. Holden remarks, the history of the instrument extends from this date until the year 1811. The work was carried on assiduously with no further interruption than was occasioned by the removal from Clay Hall to Slough, where, soon after arrival, Herschel began to lay the foundation of the whole structure, and the highly-polished speculum was put into the tube, and the first view through it was obtained on February 19, 1787. But he dates the completion of the instrument from a much later period, for the first speculum came out thinner than was intended, and from its weakness did not permit of a good figure being given to it; a second mirror, cast in January, 1788, cracked in cooling; but in the next month it was re-cast and proved of the proper degree of strength. In October following a pretty good figure and polish had been assured, and Herschel says he observed the planet Saturn with it; he continued to work upon it till August 27, 1789, when upon trial on the fixed stars it gave a pretty sharp image, and on the following night he records, "Having brought the telescope to the parallel of Saturn, I discovered a *sixth* satellite of that planet, and also saw the spots upon Saturn, better than I had ever seen them before, so that I may date the finishing of the forty-feet telescope from that time." The diameter of the polished surface of the great mirror was 48 inches. In proof of the efficiency of the mechanism for giving horizontal and vertical motions to so large an instrument he mentions that in the year 1789 he had many times taken up Saturn two or three hours before meridian passage and kept the planet in view with the greatest facility till two or three hours after the passage. On the 17th of September a *seventh* satellite of Saturn, the minute object now called *Mimas*, was discovered with the forty-feet telescope, and though the instrument was used for various purposes till 1811, these discoveries of satellites constitute its most prominent additions to our knowledge. Sir John Herschel has stated that the entire cost of construction, including the apparatus for casting, grinding, and figuring the mirrors, of which two were constructed, amounted to 4000*l.*, which sum was provided by King George III. His father observed the great nebula of Orion with the forty-feet telescope on January 19, 1811, and this was one of his latest observations. In 1839 the wood-work had so far decayed as to be dangerous, and Sir John Herschel pulled it down, but piers were erected upon which the tube was placed. Writing in March, 1847, he remarks that it was so well preserved that "although not more than one-twentieth of an inch thick, when in the horizontal position it sustained within it all my family, and continues to sustain inclosed within it, to this day, not only the heavier of the two reflectors, but also all the more important portions of the machinery."

As Prof. Holden remarks, and a similar opinion has been expressed previously, it is probable that the general public expected more from the forty-feet telescope than it actually performed; but Herschel gave valid reasons why he did not make more extended use of the instrument: the time required to get it into proper working order and

[1] Continued from p. 455.

the number of assistants necessary were impediments in the way of its being utilised for regular observation, and he assures us he "made it a rule never to employ a larger telescope when a smaller will answer the purpose." It is certain that the mirror which was in the tube in October, 1789, the month following that in which Herschel dates the completion of the telescope, was of excellent definition. On the 16th of that month he followed the sixth and seventh satellites (*Enceladus* and *Mimas*) up to the limb of the planet, and witnessed their occultation. Holden writes: "I have never seen so good definition, telescopic and atmospheric, as he must have had on these occasions."

Between the years 1796 and 1799 Herschel made an elaborate classification of stars visible to the naked eye according to their comparative brightness, which he communicated to the Royal Society in four papers published in the *Phil. Trans.* It formed the first general catalogue of the kind, exhibiting the exact state of the sky in his time. A reduction of Herschel's observations was undertaken by Mr. C. S. Peirce, and the results appear in vol. ix. of the *Annals* of the Observatory of Harvard College. So far as we know, their reduction had not been previously attempted. Instances of variability in the light of naked-eye stars were detected during the progress of the classification, the most notable discovery in this direction being perhaps that of the periodical fluctuations of α Herculis, in about sixty days. Another star in the same constellation he considered had totally disappeared in 1791, though he had seen it distinctly in 1781 and 1782.

Herschel was led to his numerous discoveries of double stars by his expectation of being able to determine the parallaxes of stars from measures made at opposite seasons of the year of the distances of pairs which appeared near together, and in the search for such pairs, his first catalogue of upwards of 200 double stars was formed and presented to the Royal Society in 1782. Long had previously measured stars upon a similar plan without success, but Herschel pointed out that his stars were not well chosen.

For the successful application of the method it is necessary that one of the pair of stars should really be situated at a much greater distance from us than the other, and as the most reasonable test of distance, Herschel assumed their difference of brightness, so that he sought for pairs where the components differed widely in this respect. The view therefore which he adopted at this time with respect to two stars seen in close proximity to each other was that one was in nearly the same line of sight as the other, but might be far more distant, thus constituting together what we now term an *optical* double star. From this beginning he was led to the discovery of revolving double stars, stars changing their relative position from year to year; and in 1803 he communicated to the Royal Society his memorable paper: "An account of the changes which have happened during the last twenty-five years in the relative situation of double stars, with an investigation of the cause to which they are owing." He was then satisfied that there were in the heavens pairs of stars which were physically connected with each other. The research for stellar parallax was not successful, but in place of it he discovered the existence of binary systems. He could not in his day decide whether the motions of suns round suns was obedient to the laws of gravitation, but five years after his death the French astronomer Savary proved that one of these revolving double stars, discovered by Herschel, ξ in Ursa Major, really was subservient to that law, and as every student of astronomy will be aware, the number of physically connected systems where the elements of the orbits have been determined, is now a large one, and is gradually increasing.

Following at present the order in which Prof. Holden refers to the scientific labours of Herschel, we now arrive at his researches on planets and satellites, respecting which the improvements he made in the construction of telescopes enabled him to advance knowledge so greatly. He was not particularly occupied with the inferior planets, but he determined the time of axial rotation of Mars with greater precision than before, and also the position of his axis. The times of the rotation of the satellites of Jupiter were found from observations on their changeable brightness, and Herschel also remarked the as yet imperfectly explained phenomena attending the transits of the satellites across the disk of the planet. Saturn, as Holden remarks, was the object of his constant attention: in addition to the discovery of the interior satellites *Enceladus* and *Mimas*, he left upon record an extensive series of observations of the seven attendants upon Saturn at that time known, and determined the time of rotation of the outer satellite *Japetus* upon its axis, by similar observations to those made upon the satellites of Jupiter. He ascertained the time of axial rotation of Saturn, and was the first who had succeeded in effecting this in a reliable manner. He also remarked the curious square-shouldered appearance which the globe of the planet has been suspected to present, and of which we still occasionally hear, though it was long ago proved by Bessel to be an illusion. It is remarkable that notwithstanding Herschel's frequent scrutiny of the planet, with all his experience of observation and the advantages of optical means surpassing by far those of his contemporaries, he does not appear to have at any time suspected the existence of the interior obscure ring. He proved beyond doubt that *Uranus* was attended by two satellites, and believed he had observed four others, and for a long time on his authority the planet was credited with six attendants.

In 1795 Herschel communicated to the Royal Society a memoir upon the nature and construction of the sun and fixed stars. As to the former he adopted a modified view of the theory which had been advanced by his friend Wilson of Glasgow; he regarded the sun as consisting of three essentially different parts: a solid and non-luminous nucleus, cool and perhaps capable of habitation, above it the atmosphere proper, and still higher the clouds or bodies which cause the sun's intense brilliancy. In this paper occurs a remark which, as Prof. Holden observes, has often been brought to bear, in consideration of the causes which maintain the solar light and heat. "Perhaps," he says, "the many telescopic comets may restore to the sun what is lost by the emission of light." We know that however credible in his day points in his theory have given way under our greatly advanced knowledge.

One of the discoveries, or perhaps we should rather say

demonstrations, which especially mark his powers of research and reasoning, was that of the motion of the sun and solar system in space and the direction of this translation, which, considered generally, has received confirmation from more recent and refined investigation. Maskelyne had determined the proper motions of a limited number of the brighter stars, and Lambert, Mayer, and Bradley had thrown out ideas upon the subject, and, following up their suggestions, he showed that the sun was really in motion towards a point in the constellation Hercules, and assigned "the apex of solar motion" with what Holden considers an astonishing degree of accuracy. His second paper on this subject (1805) his biographer views as "the best example that can possibly be given of his marvellous skill in reaching the heart of a matter, and it may be the one in which his philosophical powers appear in their highest exercise."

To gain a knowledge of the "Construction of the Heavens," as Herschel termed it, of the laws of distribution of the stars generally, the star-clusters and nebulæ in space, was confessedly a main object of his astronomical labours, and the memoirs bearing upon this subject extend over the whole period of his scientific career. For this purpose he adopted a system of *star-gauging*, which in practice consisted in pointing his 20-feet reflector towards various parts of the sky and counting the number of stars in a field of view 15′ in diameter. In this way, by methodical observation, the great differences in number of the stars in certain portions of the sky over those in other directions were reliably defined, and in extreme cases the difference was very marked, as in one mentioned by Holden, where in R.A. 19h. 41m., N.P.D. 74° 33′, in the constellation Sagitta, the number of stars per field was found to be 588, while in R.A. 16h. 10m., N.P.D. 113° 4′ in Scorpio it was only 1·1—"*ein Loch im Himmel!*" In this part of his review the author briefly touches upon the views entertained by Herschel at various periods between 1784 and 1817; he considers that while at the commencement of his researches the whole subject was in utter confusion, as they progressed data for the solution of some of the most important questions were accumulated, and the results of Herschel's whole labours form the groundwork upon which future investigators must build. "He is the founder of a new branch of astronomy."

The researches for a scale of celestial measures, on light and heat, &c., on the dimensions of the stars, on the variable emission of light and heat from the sun, are briefly referred to. Herschel's observations on the spectra of the fixed stars have been, we believe, very much overlooked. In his memoir in the *Philosophical Transactions* for 1814 he mentions that in 1798 he made some experiments on the light of a few of the stars of the first magnitude, by a prism applied to the eye-glasses of his reflectors, adjustable to any angle and direction, with the following results:—The light of *Sirius* consists of red, orange, yellow, green, blue, purple, and violet; α *Orionis* contains the same colours, but the red is more intense and the orange and yellow are less copious in proportion than they are in *Sirius*. *Procyon* contains all the colours, but proportionally more blue and purple than *Sirius*. *Arcturus* contains more red and orange, and less yellow in proportion than *Sirius*. *Aldebaran* contains much orange and very little yellow. α *Lyræ* contains much yellow, green, blue, and purple." Holden suggests that if we were to attempt to classify these stars by Herschel's observations alone we should put *Sirius* and *Procyon* into one type of stars, which have all the colours in their spectra; *Arcturus* and *Aldebaran* would represent another group, with a deficiency of yellow and an excess of orange and red in the spectrum; α *Orionis* would form a type of those stars, with an excess of red and a deficiency of orange; and α *Lyræ* would represent a sub-group of the first class. The correspondence with Secchi's types and representatives is almost complete.

There remains one other great section of Herschel's researches and discoveries, that relating to the nebulæ and clusters of stars. When he commenced his observations in 1774 very few of these objects were known. Messier's catalogue of sixty-eight such objects did not appear till 1784, and they were chiefly objects found in his long-continued search for comets. Lacaille contributed twenty-eight from his observations at the Cape of Good Hope. Herschel discovered more than 2500, which he distributed in classes as follows:—Class I. "Bright nebulæ" (288 in all); II. "Faint nebulæ" (909); III. "Very faint nebulæ" (984); IV. "Planetary nebulæ" (79); V. "Very large nebulæ" (52); VI. "Very compressed and rich clusters of stars" (42); VII. "Pretty much compressed clusters" (67); VIII. "Coarsely scattered clusters" (88). In addition he pointed out large spaces of the sky covered with very diffused and faint nebulosity, which do not appear to have been re-observed. Holden advises that they should be sought for with a powerful refractor, which would be less open to illusions than Herschel's reflectors, and that the instrument should be used in the way he adopted—in sweeping.

Throughout Prof. Holden's interesting memoir there is evinced the same enthusiastic admiration of Herschel and his scientific labours, and he concludes in the same strain. "He was born with the faculties which fitted him for the gigantic labours which he undertook, and he had the firm basis of energy and principle which kept him steadily to his work. As a practical astronomer he remains without an equal. In profound philosophy he has few superiors."

Lists of Herschel's scientific memoirs and of works bearing upon them, are appended to the volume which has formed the subject of our notice, and which, if it has a fault, is of only too limited extent to do full justice to a long life of discovery and research. We will reiterate the hope expressed by Prof. Holden in his preface, as we understand it, that some member of Sir William Herschel's family may at no distant period "let the world know more of the greatest of practical astronomers" . . . "of a great and ardent mind whose achievements are and will remain the glory of England;" and in this connection, that whatever may be found amongst his manuscripts (and as regards the drawings of the nebulæ, no less an authority than the late Prof. D'Arrest has expressed a strong hope of further publication) may at the same time be given to the astronomical public.[1]

J. R. HIND

[1] Prof. Holden's work is published in London by Messrs. W. H. Allen and Co.

BRITISH FISHES

Natural History of British Fishes: their Structure, Economic Uses, and Capture by Net and Rod. Cultivation of Fish Ponds, Fish suited for Acclimatisation, Artificial Breeding of Salmon. By Frank Buckland, Inspector of Fisheries. (London: Society for Promoting Christian Knowledge.)

IT would have been difficult for Mr. Buckland to produce a dull book on any question connected with the economy of our fisheries; his merit in this respect has tended, however, to lead him too much in an opposite direction. It is painful, now that we are deprived of the living presence of the genial naturalist and industrious fishery inspector, to write an unkind word regarding any branch of his life's work; but of this book we are compelled to say that we would have appreciated it better had it been less "familiar" and more scientific. That it should be full of interesting information about fishery matters was quite to be expected from the richness of the stores which its author always had at his command, and if Mr. Buckland had taken pains to digest the *matter* so lavishly extracted from *Land and Water*, and had likewise collated the miscellaneous information contained in the volume with care, he might then have enjoyed the satisfaction of presenting to the public a natural history of British fishes which probably would have compared satisfactorily with other good books of the kind. It is not too much to affirm that a carefully edited selection from the numerous essays contributed to the various blue-books to which the deceased gentleman was so voluminous a contributor, would have made a more interesting volume than the present work. The fact is, Mr. Buckland was nothing if he was not sketchy and rapid; he would not be tied down to severe statements, but preferred to give an off-hand opinion in a dashing way, no matter that he might find out within the year that what he had advanced was very far wrong. In the present volume, as a glance at the plethoric title-page will show, Mr. Buckland attempted too much, with the result that portions of the information conveyed are scrappy, while some of it is probably slightly imaginative: books and articles written in railway trains often enough provide hard work for the reader. In a preface to his work Mr. Buckland takes pains to point out how greatly we are deficient in *exact* knowledge of the habits of our sea-fish, of the times and places of their spawning, of the food they eat, and of the period at which they are able to repeat the story of their birth. Some of the many questions which are asked by Mr. Buckland we are under the impression he should himself have been well able to answer. Whether cods' eggs "sink or swim" has been often discussed, and the author ought to have been able to tell us the truth in that matter; but, on turning to the account given of the cod-fish in the present book (p. 50), it seems to be singularly deficient in its details of the natural history of that animal. So far as we can observe, no reference whatever is made to the theory of Sars with reference to the floating of the eggs, but a few pages relative to the personal adventures of the author are not wanting, whilst the old story of "the Logan fish-pond" is re-told with great circumstantiality. Twenty-five pages of the work are devoted to the salmon (*Salmo salar*), and the essay, confused as it is, is well worthy of perusal, although it contains, as do other portions of the book, a good deal about Mr. Buckland, and recapitulates, as usual from *Land and Water*, an account of some of the big fish in "my museum." It would be a tedious process to anatomise the contents of this "Natural History of British Fishes"; taking all that is written at its true value, we set down the work as an interesting collection of miscellanea. The account given of the Loch Leven trout (*Salmo Levenenses*) is exceedingly meagre, as is likewise the descriptions of several other fresh-water fishes, notably the vendace of Loch Maben. The most suggestive part of the present work is that which is devoted to "Pisciculture" (pp. 334 to 375). Under the title of "The Cultivation of Fish Ponds," much interesting matter is given, and a good deal of information that must be new to the uninitiated is set forth. But notwithstanding the many pleas for pisciculture which have at various times been advanced, it is questionable if the cultivation of other fresh-water fish than the salmon would *pay* as a food resource. A larger supply of trout would no doubt be welcome to the angler, because the trout is *the* fish of the angler *par excellence;* moreover in many places angling has now to be paid for, and lairds in Scotland who let their moors and lochs can always lease them to greater advantage when they are well stocked.

OUR BOOK SHELF

Proceedings of the Aberdeenshire Agricultural Association. (Fourth Annual Report, 1879-80.)

WE have here an account of the field and laboratory experiments carried out by Mr. Jamieson for the Aberdeenshire Association during the year 1879. The crops experimented on were turnips and oats. As before, the principal object in view was to ascertain the comparative manuring value of various phosphates in different states of aggregation. We can glance at only a few points in the results.

Mr. Jamieson claims to have shown that a finely powdered mineral phosphate, as, for instance, powdered coprolite, is nearly equal as a manure for turnips to the same amount of phosphate applied in a soluble form as a superphosphate, while the simply powdered phosphate is of course much cheaper than the manufactured manure. There is probably no doubt that on some soils a finely powdered mineral phosphate is sufficiently soluble to produce a considerable effect on the crop, if only the phosphate is applied in sufficient quantity, so as to present a considerable surface for attack; and to Mr. Jamieson belongs the credit of giving prominence to this fact, though it was by no means unknown before his experiments. There is however no reason for supposing that dissolved and undissolved phosphates have the same manurial value. When large doses of each are applied the manures may appear of equal value, because while the undissolved phosphate is sufficient for the wants of the crop, the dissolved phosphate is in excess of all requirements, and is therefore wastefully employed. Mr. Jamieson applies 100 lbs.[1] of phosphoric acid per acre both as dissolved and undissolved phosphate; that is to say, about 3 cwts. of bone ash and 5 cwts. of bone-ash superphosphate. Such a comparison is probably quite unfair to the soluble phosphate. For the small turnip crops obtained in Mr. Jamieson's experiments 2½ cwts. of

[1] On page 15 of the appendix the amount of phosphoric acid applied per acre is stated to be 100 lbs., but on page 16 the quantity is given as 200 lbs.

superphosphate drilled with the seed would be found quite sufficient, and probably fully equal in effect to twice the quantity of phosphoric acid applied as powdered coprolite.

Phosphate of iron applied alone was found to have practically no effect on the turnip crop, and the effect of phosphate of aluminium was but little; this is pretty much as we should expect. There is apparently some mistake in the printed analysis of the phosphate of aluminium used, as it is made to contain 38·28 per cent. of lime, and only 4·76 per cent. of ferric oxide and alumina.

The analyses given of the turnip soils cannot pass without a word; the reporter is surely unaware of the absurdity which these analyses present. The soil of the unmanured plot in the five experimental fields was analysed in 1876, and again in 1879, after three turnip crops had been taken. The analyses show that on an average about 20 per cent. of the nitrogen, and about 48 per cent. of the phosphoric acid in the soil had been removed during these three years, and yet the total weight of the three turnip crops grown on the five fields during this period averaged but 16 tons per acre! The only remark made by the reporter on these figures is that the soil has evidently become reduced in nitrogen, and much reduced in phosphates; the fact that either the soil sampling or the analyses must be utterly wrong seems to have altogether escaped his attention.

The experiments with oats do not call for any special remark, except to note the patience which shelled 136,000 grains by hand in order to determine the proportion of kernel to husk in the produce of the various plots.

May we suggest that in a report of field experiments the dates of sowing and of harvest should always be given, and also a description of the character of the weather during the growing period. Without such facts before us it is impossible to interpret the results of field experiments.

Proceedings of the London Mathematical Society. Vol. xi. (November, 1879, to November, 1880).

THIS is a smaller volume than usual, there being fewer papers, and none of them of a great length. The pure mathematics prevails somewhat more than usual over the mixed.

Prof. Cayley contributes articles "On the Binomial Equation $x^p - 1 = 0$; Trisection and Quartisection," a theorem in spherical trigonometry, on a formula of elimination. Sir James Cockle writes "On a Binomial Biordinal and the Constants of its Complete Solution." Mr. J. W. L. Glaisher, "On a Method of obtaining the q-formula for the Sine-amplitude in Elliptic Functions"; Mr. H. W. Lloyd Tanner, "Notes on a General Method of Solving Partial Differential Equations of the First Order with several Dependent Variables," and a preliminary note on a generalisation of Pfaff's Theorem; Mr. J. J. Walker, "Theorems in the Calculus of Operations"; and Mr. T. R. Terry, "Notes on a Class of Definite Integrals." Papers of a geometrical nature are—Mr. J. Griffiths, on a geometrical form of Landen's theorem with regard to a hyperbolic arc, and on a class of closed curves whose arcs possess the same property as two Fagnanian arcs of an ellipse; Mr. H. Hart, on the focal curves of a bicircular quartic; Mr. H. M. Taylor, on the equation of two planes which can be drawn through two given points to touch a quartic; Rev. J. Wolstenholme, a form of the equation determining the form and directions of a conic whose equation in Cartesian co-ordinates is given. Dr. Klein of Leipsic has a short note on the transformation of elliptical functions; Mr. Greenhill applies elliptic co-ordinates and Lagrange's equations of motion to Euler's problem of two centres of force; and Mr. Routh writes on functions analogous to Laplace's functions. Lord Rayleigh's papers are on reflection of vibrations at the confines of two media between which the transition is gradual, and on the stability or instability of certain fluid motions. Mr. Samuel Roberts has two notes: one on a problem of Fibonacci's, and the other on the integral solution of $x^2 - 2Py^2 = -z^2$ or $\pm 2z^2$ in certain cases; Mr. R. F. Scott writes on cubic determinants and other determinants of higher class, and on determinants of alternate numbers (a treatment which he has adopted in his work on "Determinants"). Mr. Hugh McColl contributes a fourth paper on the calculus of equivalent statements (cf. Prof. Jevons's remarks, NATURE, vol. xxiii. p. 485). Other minor articles conclude the volume.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The New Museum of Natural History

THE new Natural History Museum, opened on Easter Monday, was visited by some 16,000 people of a most orderly and respectable class. Owing to the great exertions of Dr. Woodward, whose zeal is beyond praise, the main gallery, the Pavilion, and the Gallery of Reptiles were shown in a practically completed state. The Mineral Gallery has long been ready, but the arrangement of the botanical section is still incomplete, and it was entirely closed. Some little trouble was caused with the umbrellas, and it might be worth while to consider whether, except perhaps in wet weather, the umbrellas need be taken away. The idea that people poke with sticks at objects in museums has been long exploded, and no inconvenience is felt at the Kensington Museum, the Louvre, and nearly all foreign galleries and exhibitions, where umbrellas are admitted.

The architecture in the Mammalia Gallery is very obtrusive, and its over-ornate character and the variety of tone of the terra-cotta, and the similarity of this in colour to the skulls and skeletons of the fossil mammalia, are most unfortunate.

It seems a pity that some style with more repose than "Decorated Norman" was not selected. Although very beautiful as a building, and with many features deserving high praise from an architectural point of view, it is evidently not the style best adapted to set off natural-history specimens. The cathedral-like Index Museum, with its rather dark side-chapels, and the Museum of British Zoology are of proportions that will render it difficult to make an effective display in them.

I hope that it is not finally decided to place the recent mammalia on the first floor and the birds on the ground floor, because the architect's string courses would be interfered with otherwise by the cases. The living and extinct mammalia should face each other, and the birds go aloft. Convenience has already been too much sacrificed to architecture. Every time the first floor is visited the length of the Index Museum, 150 feet, must be traversed to reach the stairs, and the same distance back along the corridor to reach the door of the Mineral Gallery. This means an immense waste of time. I also notice that the crane is close to the main entrance, and that there are no proper lifts.

If it was necessary to fashion all the ornaments from natural-history objects, it is a pity that the restorations were not accurately made. The oft-repeated figure of a Dapedius swallowing a fish almost its own size, and of spiral shells bent to accommodate them to the mouldings of an arch, is not instructive. The humour of ornamenting (?) the arch leading into the pavilion with a hideously-represented Archæopteryx in high relief, repeated a dozen times, is not obvious, but some joke must doubtless be intended.

The cost of the small bronze and glass conservatories in the botanical department is out of all proportion to the objects they are to contain. Dried stems of tree-ferns and palms, though very interesting in their way, do very well in other museums without glass cases, and can be replenished for next to nothing.

F. G. S.

The Tide-Predicter

MR. EDWARD ROBERTS' letter in NATURE for April 14 contains statements giving an erroneous view of the origin of the tide-predicter. Any one who feels sufficient interest in the subject to derive full information will find it in my paper on "The Tide-Gauge, Tidal Harmonic Analyser, and Tide-Predicter," read before the Institution of Civil Engineers on March 1 and in the abstract of the discussion which followed it, to be published in the *Minutes of the Proceedings* of the Institution (vol. lxv. sess. 1880-81, part iii.), and he will see that my letter in NATURE of March 31 is correct. WILLIAM THOMSON

The University, Glasgow, April 16

Geological Relations of Gold in Nova Scotia

IN the notice of the report of Mr. Murray on the gold of Newfoundland (NATURE, vol. xxiii. p. 472) I observe a reference to my own opinion of the age of the gold of Nova Scotia which needs some correction. In the second edition of "Acadian Geology" (1868) the gold-bearing series is included in the Lower Silurian, but this referred to the larger sense of that term in which it was used to include the Cambrian as well. In the third edition (1878, Supplement, pp. 81, 85, 92) I have referred this formation, on the evidence of fossils and stratigraphical position, to the age of the Lower Cambrian or Longmynd series, thus placing it on a lower horizon than the fossiliferous Primordial of Eastern Newfoundland, which I suppose to be of the age of the Acadian or Menevian group. There is therefore little difference between Mr. Murray's estimate of the age of the gold-bearing rocks of Newfoundland and my own of that of the similar rocks in Nova Scotia, except that I presume he would classify the Newfoundland series as Upper Huronian rather than Lower Cambrian. With reference to this I have been disposed to regard Mr. Murray's *Aspidella* slates and the associated rocks as equivalents of the Kewenian or "Upper copper-bearing group" of the West, and probably Upper Huronian, in which case they might be a little below my Nova Scotia Lower Cambrian; but the precise age of both series is determined merely by the fact that they appear to belong to the period between the Huronian proper, or Lower Huronian, and the Acadian group, or Menevian (Etage C. of Barrande).

It is proper to add that in the third edition of "Acadian Geology" I have shown that the filling of the Nova Scotia gold veins is much more recent than the containing rocks, and belongs to the time intervening between the Upper Silurian and the Lower Carboniferous, the richer deposits also appearing to be related to the occurrence of intrusive granites of Devonian age. There is no reason, therefore, other than the mineral character of the containing beds, why such veins might not occur in any rocks older than the Devonian, and gold discoveries have been reported in localities where the rocks are supposed to be Huronian and Silurian; but I have had no opportunity of personally verifying these statements. Thus far the important gold veins are known only in that great series of slates and quartzites of the Atlantic coast which I have referred to the Lower Cambrian. J. W. DAWSON

McGill College, Montreal, April 4

Symbolical Logic

PROF. JEVONS, in his criticism of my method in NATURE, vol. xxiii. p. 485, has stated the main points at issue between us so fully and clearly, and on the whole so fairly, that I need only say a very few words in reply.

As to the charge that my method is ante-Boolian or anti-Boolian, I do not seek to repel it; on the contrary, I maintain that my method is different from Boole's in principle, and very different indeed in its practical working. The really important questions to be settled are these:

1. Are the definitions which I give of my symbols clear and unambiguous?

2. Are the rules and formulæ which I derive from these definitions correct?

3. Are the innovations which I propose of any practical utility?

Now, I do not think that any one who has read my papers in the *Proceedings* of the London Mathematical Society and my articles in *Mind* and in the *Philosophical Magazine* will refuse to answer *Yes* to questions 1 and 2; and with regard to question 3 I can only say that any one who answers *No* is bound in fairness to prove the inutility of my innovations by solving one or two of my hardest problems without their aid, and in an equally clear and concise manner. My proposal of an amicable contest in the *Educational Times* meant nothing more serious than this.

Some of my critics (not including Prof. Jevons however) seem anxious to magnify the points of resemblance between my method and its predecessors, especially Boole's, and to minimise the points of difference. It may be as well therefore to state briefly what characteristics distinguish my method, so far as I know, from all the methods which have preceded it, and what advantages, in my opinion, accompany these characteristics.

In the first place, then, every single letter in my notation, as well as every combination of letters, denotes a *statement*. By this simble device I gain the important advantages of generality of expression and uniformity of interpretation and treatment. It enables me to express many important logical laws in simple and symmetrical formulæ, as, for instance,

$$(A:a)(B:b)(C:c):(A+B+C:a+b+c),$$

which otherwise could not be so expressed. To secure these advantages I sacrifice absolutely nothing. The relations of classes, including the ordinary syllogisms, I express by speaking throughout of *one individual*, just as mathematicians express the properties of curves, surfaces, and volumes, by speaking throughout of the varying distances *of one representative point*.

My claim to priority on this head has been called in question on the ground that Boole too, in his equations about "secondary propositions," denotes statements by single letters. The plain truth however is that Boole takes some pains to prevent his readers from imagining that he does anything of the kind. He says distinctly, and in perfect consistency with the whole tenor of his book, in which he describes his algebra of logic as a mere offshoot and part of the ordinary algebra of quantity, that in his equations any single letter, such as x, denotes the *portion of time* during which some proposition x is true, the whole universe of time to which the discourse refers being the unit (see "Laws of Thought," from p. 164 to p. 170). Neither will one find anywhere in Boole's work the idea (suggested to me by analytical geometry) of investigating the relations of different classes, while speaking only of *one individual*, and thus dispensing entirely with the quantitative words *all*, *some*, and *none*, which are so characteristic of the old logic.

Another peculiarity of my method is that my symbol of denial (an accent) is made repeatedly to apply to expressions of varying complexity, as, for instance, $(xy)'$, $(x+yz)'$, $(x:y')'$, leading to rules and formulæ of operations, to which I find no parallel in any prior symbolic system with which I am acquainted.

Boole uses $\bar{x}$ as an abbreviation for $1-x$. Let those who insist that Boole's horizontal stroke is exactly equivalent to my accent express in his notation the complex equation

$$(x=y)'=(x:y)'+(y:x)',$$

and explain its meaning clearly *without departing from Boole's quantitative interpretation of his symbols*.

Lastly, my symbol : expresses *implication* or *inference*, and does not, therefore, exactly coincide in meaning with Prof. Peirce's symbol of inclusion —<, as defined by him in his "Logic of Relatives," published in 1870. This symbol of inclusion, as I understand Prof. Peirce's definition of it, is simply equivalent to the words "is not greater than," and is therefore restricted to number and quantity. It is true that Prof. Peirce in his recent memoir on the "Algebra of Logic" extends the meaning of this symbol of inclusion, so as to make it also convey the same meaning as my symbol of implication; but as this memoir was published subsequently to my second and third papers in the *Proceedings* of the Mathematical Society, to which Prof. Peirce explicitly refers in his memoir and accompanying circular note, this later definition does not bear upon the point in discussion.

Prof. Jevons objects to my $\alpha:\beta$ as an abbreviation for $\alpha=\alpha\beta$, because he thinks it obscures the real nature of the reasoning operation. But one might with equal justice object on the same grounds to a^3 as an abbreviation for aaa, or to the left side of the equation in the binomial theorem as an abbreviation for the right side. The symbol $\alpha:\beta$ is the exact equivalent of $\alpha=\alpha\beta$, just as $\alpha=\beta$ is the exact equivalent of $(\alpha:\beta)(\beta:\alpha)$, and I do not see that I create any obscurity by adopting in any investigation, and at any stage of the investigation, whatever form seems most suitable for the immediate purpose in view. But whether I am right or wrong in this opinion can only

be decided by actual examination of my published papers on symbolical logic, of which Prof. Jevons has very kindly given in NATURE a full and complete list. HUGH MACCOLL

73, Rue Siblequin, Boulogne-sur Mer, April 7

Agricultural Communism in Greece

THE article in NATURE, vol. xxiii. p. 525, on Aryan villages and other Asiatic communities reminds me of what I saw in 1843 in the course of a journey through Greece. On St. George's Day, a high festival with the Greek peasants, when crossing the range of Mount Cithæron between Thebes and Eleusis, I saw my companion, who was about half a mile ahead, surrounded by a number of men, and then pulled from his horse. The man we had engaged as interpreter, guide, and protector, the "dragoman," bolted as a matter of course, thinking we had fallen upon a nest of brigands; but when I reached the scene of action I was surprised to find that the yelling and uproar heard in the distance were not murderous nor at all malignant, but purely hilarious. I was dragged from my horse also, and surrounded by about twenty young fellows with shaven heads and long scalp locks, half stripped, half drunk, and very dirty, but perfectly good-humoured.

We were presently made to join in a wild dance, a survival of the Pyrrhic dance of antiquity, which we improved very successfully; my companion, C. M. Clayton, from Delaware, doing a nigger break down and I the sailor's hornpipe.

On the final arrival of our dragoman we learned that the twenty young men were brothers, and that the old man with long white beard who sat gravely looking on and playing a sort of tom-tom to tune the dance was their father. On our expressing surprise at so large a family of sons being so nearly of the same age he explained that ἀδελφός did not always signify a blood relation, and that these were merely *agricultural* brethren. They were the united proprietors or renters (I do not remember which) of the adjoining farmhouse and the surrounding land, which they cultivated under the direction of the old man whom they had selected as their father, who was entrusted with the custody and division of their capital and profits, who arbitrated in cases of quarrels, and was otherwise obeyed in most things.

Here was a patriarchal form of communism that we afterwards met with in several other instances, but in this and the other cases it was limited to young unmarried men. There were no women in the dance and none visible on this farm, which was some miles distant from the nearest village, Platæa.

At that time the Klephts, or brigands, were united in similar communities, who sternly abjured all communication with the fair sex.

When we had finished our dance and paid for sufficient wine to go round the family circle we found that before going we must kiss all the brothers or give mortal and dangerous offence. Andrew, our dragoman, with the inventive facility of his nation, extricated us from this by solemnly stating that in England it was an established custom to show respect for a family by embracing the father only, and bowing separately to each of the sons.

I am unable to supply any further particulars concerning the internal economy of these communities, cannot say whether they prevail chiefly among the Greeks or the Albanians (the latter constitute a large proportion of the agricultural population of Greece), nor how they dissolve when the brothers become married or the father dies. I have met with no account of them in the course of my reading, but am not at all surprised at this, seeing how profound is our general ignorance of everything pertaining to Greece, an ignorance which is most glaringly displayed by political writers and others, who speak of Athens as though it were Greece, and of Athenian proceedings as though they were the action of the Greeks.

But for the accident of this rather startling festive encounter with these brethren on this particular holiday, we might have travelled for weeks without meeting any visible indications of such fraternities. We should have passed the brothers if they were working in the fields, and the patriarch had he been sitting alone at the farmhouse door, without special notice. It was only after our curiosity had been excited that we discovered other patriarchs and other brethren by special inquiry where their existence was vaguely indicated.

Among the readers of NATURE there may be some who have sufficient acquaintance with the Greek people, outside of Athens, to be able to supply interesting particulars concerning these curious communities. They may be survivals of our ancient communism, or a modern device for mutual protection forced upon the rural population by the absence of any enforcement of law and social order by those who consume the taxes in Athens.

W. MATTIEU WILLIAMS

Heat of Stellar Masses

I SEND you a working hypothesis which I think will well pay for its place in the world. It is as to the heat of large stellar masses; that the imperfect conduction of the kinetic force producing gravitation through large stellar masses causes heat in them.

The quantity of heat stored up may depend partly on the proportion of mass to radiating power, and partly, perhaps, on the condition of the mass for such conduction.

Washington, D. C., March 25 SAML. J. WALLACE

Shadows Cast by Venus

ON March 21 last, about 8 p.m., I was walking among some trees by a river's bank. The ground was covered with recently-fallen snow, and the shadows of the trees were unmistakably, though faintly, traceable on the white surface. The night was dark and the shadows were thrown by Venus, which was shining with unusual brilliancy. I believe this obvious form of the phenomenon is not a common one in our latitude.

CHAS. T. WHITMELL

31, Havelock Street, Sheffield, April 18

The Sparrow and Division of Labour

THE following curious fact may possibly interest your ornithological readers:—Last year and the year previous two pairs of swallows made their nests and successfully reared their broods under the eaves of my house. Within the past fortnight a brace of astute London sparrows have apparently recognised the principle of division of labour as applicable to their requirements in the art of nest-building. They have selected the largest and most substantial of the swallows' nests referred to; and, after devoting a day or two, on starting on their enterprise, to the enlargement of the entrance hole, which was probably too narrow for them, have constructed their bed within of bits of grass and feathers in the usual fashion. They are now enjoying their honeymoon in their new quarters. G. C. WALLICH

3, Christchurch Road, Roupell Park, April 11

SIR PHILIP DE MALPAS GREY EGERTON, M.P., F.R.S.

IN Sir Philip Egerton geologists have lost one of that band of workers who placed their science upon the footing which it now occupies in this country. Unfortunately that band has been of late years greatly diminished by death. Born in 1807, Sir Philip Egerton with his old friend and fellow-worker, Lord Cole (now the Earl of Enniskillen), while still at Oxford commenced the collection of fossils, and very soon their attention was especially directed to fossil fish, of which but very little was at that time known. As specimens of this group of organisms often occur in duplicate, the individuals breaking across so that two opposite slabs each contain one-half, the two friends agreed to share their spoils, and thus both collections were enriched. When in 1840 Agassiz visited this country, intent upon his great ichthyological memoirs, he found in the museums of Sir Philip Egerton and Lord Cole an abundance of materials ready to his hand. The specimens were carefully figured, and descriptions of them included in the several great works which Agassiz successively issued. The original drawings by Dinkel are now among the archives of the Geological Society. But Sir Philip Egerton was by no means merely a collector of fossils, he was a very diligent and successful student of ichthyology. Many valuable papers on fossil fishes were written by him at different times, and a series

of papers published in the decades of the Geological Survey of the United Kingdom are among the most valuable of the works issued by that body. An excellent man of business, Sir Philip took an active part in the administration of the British Museum, the London University, the Geological Society, and other institutions for the promotion of science. All who knew him will miss the kindly face and cheerful manners which distinguished him. Only two days before his death he was in his place in Parliament, but a chill caught during the lately prevalent east winds proved rapidly fatal. At the last meeting of the Geological Society the vice-president, Mr. J. Whitaker Hulke, F.R.S., made announcement of his death, and the sudden and unexpected tidings concerning one who was so widely known and so universally respected cast a sad gloom over the proceedings of the evening.

A correspondent sends us the following additional note on the late Sir Philip Egerton:—

The knowledge of the extinct species of fishes is one of the latest additions to palæontology, and the creator of this department of the science, Louis Agassiz, found the richest materials for his great work in the British Isles. In their acquisition he was greatly aided by Lord Cole, now Earl of Enniskillen, and by Sir Philip de Malpas Grey Egerton, Bart., M.P. Their gatherings resulted in most complete collections of fossil fishes, and science is much indebted to the catalogues drawn up and published by Sir P. Egerton of that preserved at Oulton Park. Besides the species named by Agassiz this collection includes many which have been subsequently determined and described by Sir P. Egerton, whose name will be ever associated with that of Agassiz in palichthyology. In his public career Sir Philip Egerton has been distinguished by his unremitting attention to his parliamentary duties in the long period since his election in 1830. The British Museum sustains a severe loss in a Trustee, elected in 1851, whose scientific knowledge, sound judgment, and administrative ability were of the greatest value, especially to the Natural History Departments. Sir Philip's last attendance at the Board was but a few days—apparently in his usual good health—before his lamented death.

THE SCIENTIFIC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING

FOUR Cantor Lectures on this interesting subject have just been delivered at the Society of Arts by Prof. W. Grylls Adams, F.R.S.; the lectures will be published in full in the *Journal* of the Society of Arts, but we are able to give an abstract of them by Prof. Adams. In the first lecture, the discoveries of Œrsted, Ampère, Arago, and the early discoveries of Faraday on magnetic and current induction were considered in their relation to the principles of conservation and transformation of energy.

Lecture I.—Prof. Adams began by stating and illustrating the fact that important discoveries, after they are made, often pass through a stage of neglect or a stage of quiet development, then enter on the practical stage, when new facts and new inventions follow with great rapidity. The potential energy of the discoverer is transformed into energy of action in many directions with more or less efficiency, according to the retarding state of the medium through which that action takes place.

Electrical science has passed through these stages, whether we regard telegraphy from the work of Sir Francis Ronalds in 1816, who said, "Let us have electrical *conversazione* offices communicating with each other all over the kingdom," down to the establishment of telephonic exchanges, or whether we consider electric lighting from the grand experiment of Sir Humphry Davy in 1813 with a battery of 2000 cells, down to the latest results obtained by means of the most recent magneto- or dynamo-electric machines.

In the year 1819 Œrsted observed the action of a current of electricity on a suspended magnetic needle, and in the year 1820 Ampère studied the laws of their mutual actions, and propounded his celebrated theory of magnets and of terrestrial magnetism, making magnetism the resultant action of electric currents. In the same year Arago discovered the magnetisation produced by electric currents, laying the foundation of the subject of electro-magnetism.

The discoveries of Œrsted, Ampère, and Arago were fully illustrated by experiments, and their connection with one another explained. In the same year, 1820, Schweigger invented the galvanometer, and in 1827 Ohm deduced his simple theory of the action of batteries from the principle of Volta.

The relation of the experiments of Œrsted, Ampère, and Arago to the principle of conservation of energy was then fully considered. Considering Ampère's experiment of the motion of wires towards one another when like parallel currents are flowing in them, it was shown that the currents must be diminished whilst they are actually approaching, and increased whilst they are separating, and so by supposing one of the original currents very small, the relation between Ampère's results and the induction of a current by moving a wire in the neighbourhood of another current was deduced.

The laws of induced currents were then explained and illustrated by some of the early experiments of Faraday, who discovered the induction of electric currents by magnets in 1831.

"In his first series of papers to the Royal Society entitled—(1) On the Induction of Electric Currents, (2) On the Evolution of Electricity from Magnetism, (3) On a New Electrical Condition of Matter, (4) On Arago's Magnetic Phenomena, Faraday unfolds step by step the laws of the induced current in a helix of wire B, placed near to another helix A, carrying a voltaic current.

"That as long as a steady current was maintained in A there was no current induced in B; that on making contact in A or on approaching the wires there was a momentary inverse current in B, and on breaking contact in A or on separating the wires, there was a direct induced current in B. That as this current was of the nature of an electric wave like the shock of a Leyden jar, it might magnetise a steel needle, although it produced slight effect on a galvanometer, and how this expectation was confirmed, and that the needle was magnetised opposite ways on making and on breaking contact." Then in his evolution of electricity from magnetism he gives an account of the greatly increased effects on introducing soft iron cores into his helices of wire, and shows that similar effects are obtained by using ordinary magnets in place of a helix carrying a battery current round an iron core, *i.e.*, in place of an electro-magnet. He then describes the experiment of introducing a magnet into a coil of wire, and shows that the same current is obtained whether the marked end of the magnet be introduced at one end of the coil or the unmarked end introduced at the other, and that a current is produced in the opposite direction to the former on withdrawing the magnet from either end. Then after describing the method of producing his induction spark and also muscular contractions of a frog by means of a loadstone and coil, and remarking that the intensity of the effect produced depends upon the rate of separation of the coil from the poles of the loadstone, he concludes this section thus: An agent which is conducted along metallic wires in the manner described; which, whilst so passing, possesses the peculiar magnetic action and force of a current of electricity; which can agitate and convulse the limbs of a frog, and which finally can produce a spark, can only be electricity.

Faraday also observed the difference of time between induction by a battery current in a coil, which is instantaneous, and induction by a magnet, which requires an interval of time to get up to its full value; and accounted for this retardation by supposing that there is a redistribution of the Amperian currents in the iron itself, so that the magnet requires time to rise to its full power.

Little did Faraday dream of the rapid development and the marvellous results which were to flow from his experiments when, fifty years ago, he established the laws of magnetic and current induction, being stimulated (as he says) to investigate experimentally the inductive effects of electric currents with the view of elucidating Ampère's beautiful theory of magnetism, and in the hope of obtaining electricity from ordinary magnetism.

Lecture II.—It was shown in the last lecture that a circular current or a current in a coil of wire acted as a magnet, one face of the coil, in which, as we look at it, the current appears to go contrary to the hands of a watch, corresponding to the marked pole or the pole of a magnet which points to the north, and the opposite face of the coil corresponding to that pole of the magnet which points to the south. Each of the Amperian circular currents in the separate molecules of a magnet is equivalent to a fine magnet with poles of the same magnetic strength as the current, and occupying the same position, and the collection of Amperian currents will have the same magnetic effect as the bundle of small magnets, each of which gives the direction of the magnetic force at the point. These separate fine magnets may be regarded as Faraday's lines of force, and the number of them issuing from a magnetic pole will be a measure of the strength of the magnet. The magnetic field of the magnet is any portion of space to which the influence of the magnet extends. The current which will be produced by the motion of a conducting wire in the magnetic field will be proportional to the strength of the magnetic field, *i.e.* proportional to the number of lines of force cut by the conductor; so that the current produced in each half turn of a coil of wire revolving on an axis is proportional to the number of lines of force cut by the coil during its rotation, so that the total current from the coil will be proportional to the number of lines of force multiplied by the number of times the wire is repeated in the coil. In the case of a coil of wire rotating in the field of a magnet, if the axis of rotation is parallel to the lines of force no current is produced, but as the axis of rotation is turned more and more nearly at right angles to the lines of force the current in the coil is increased. Taking the earth for our magnet, when the axis of rotation is perpendicular to the lines of force and still in the magnetic meridian, the current in that half of the coil which is moving from west to east will be from north to south, and the current in the other half of the coil which is moving from east to west will be from south to north, so that in the whole coil we get during every half turn an all-round current in one direction in the coil. The direction of the current in the coil, as we look at it from the east, is the same as the direction of rotation of the coil as we look at it from the north. The direction of the current in the coil is alternately in opposite directions for every half turn, but a continuous current may be obtained from it by reversing the connections with the ends of the coil by a commutator at the same instant as the currents are reversed in the coil.

These are the principles of all magneto-electric machines. The distribution of lines of force in the magnetic field of currents and of magnets is well shown by projecting some of Prof. S. P. Thompson's transparencies, which show the magnetic effects resulting from the mutual action of currents and magnets on one another.

After the discovery by Faraday, in 1831, of the method of producing a current of electricity by the sudden removal of a coil of wire from the pole of a magnet, the laws of these currents were being developed, but for twenty years no attempt was made to apply them for the purposes of electric lighting. Voltaic batteries were being improved, and the more constant batteries of Daniell, of Grove, and of Bunsen were discovered, and these were the sources of electricity employed to produce the more powerful currents of electricity. In this country forty or fifty cells of Grove have given us the electric light for optical experiments in our laboratories, and the light was kept steadily in the same position by the elaborate arrangements of wheel-work and electromagnets devised by Staite in 1847 and by Foucault, which have reached very great perfection in the hands of Duboscq. In the Duboscq lamp the current passes always in the same direction, and the positive carbon becomes hollowed out, and burns away about twice as fast as the negative carbon, which becomes pointed. The carbons are moved towards one another by means of a drum carrying two wheels, whose diameters are as 2 to 1, which move two racks which bring the carbons together This lamp is especially adapted for projection on a screen, and we may study the forms of the carbons by projecting them, and also study the kind of light given out by the vapours of metals burning in the arc; if we burn silver in the arc we shall see that it is rich in the violet or chemical rays, which points to the reason why the salts of silver are so much used in photography.

Even with constant batteries there is great variation in the steadiness of the electric light, but much more is this the case when the current of electricity is obtained by the motion of a coil of wire in a magnetic field, for every alteration in the resistance in the circuit reacts on the machine producing the current so as to increase the disturbance. Hence regulators are necessary in order to control the current, so as to keep the light constant. In electric lighting regulators may act on the electric lamps themselves so as to give a steady current between the carbons by keeping them the same distance apart, or regulators may be used in another part of the circuit to control the current automatically by causing it to introduce extra resistance when the current increases, and to diminish the resistance when the current diminishes.

Various methods of regulating the current, including those employed by Siemens, Lane Fox, and Edison, were then described. In order to find the yield or effective work of batteries or magneto-electric machines and their efficiency, measurements of the current and of the work done by the current must be made.

There are four principal methods of measuring electric currents:—

1. The galvanometer method, by which with a galvanometer of small resistance a very small fraction of the current is measured, and any error of observation is multiplied in estimating the total current flowing.

2. The heat method, in which the current is measured by the heat developed by the current in a given resistance in the circuit according to Joule's law, that the heat is proportional to the square of the strength of current.

3. The electrometer or potentiometer method, in which the difference of potential between two points in the circuit with a given resistance between them is directly measured and the current deduced from Ohm's law.

In using Thomson's quadrant electrometer for strong currents, the needle and one pair of quadrants should be connected together, so that the deviation is then proportional to the square of the difference of potential, and this method is applicable for continuous currents and also for alternate currents. By means of two electrometers in different parts of the circuit the current and also the work done by it may be at once measured. If, for instance, one of the electrometers be connected with the two carbons, the difference of potential and work done between the carbons may be determined. By such measurements it has been found that there is an electro-

motive force of 30 or even 40 volts between the carbons in the electric arc, but that the actual resistance of the arc is small.

4. The electro-dynamometer method, the best method fitted for ready measurement, in which the current in one coil is attracted by the same current flowing in another coil, and the attraction is balanced by a spring as in Siemens's electro-dynamometer, or by weights as in Trowbridge's electro-dynamometer. A beautifully-made instrument by Elliott and Co. was exhibited, in which the coils are thick copper bands, fixed coils being placed on each side like the coils of a tangent galvanometer, the suspended coil being placed between them in place of the magnet of the galvanometer. This instrument is especially useful for the measurement of very large currents in absolute measure.

The remainder of the second lecture was devoted to the consideration of the efficiency of batteries or of magneto-electric machines when employed as motors to do work by means of electricity, and it was shown that the greatest amount of effective work is produced when one-half the energy of the battery or current-generating machine is converted into useful work in the electric circuit. Numerous experiments were made with Clarke's and other magneto-electric machines to show that by the same machine work may be produced by sending a battery current through it, causing motion of the armature carrying the current in accordance with Ampère's laws, or a current of electricity may be generated by turning the armature, *i.e.* by doing work upon the machine, so that a magneto-machine is a reversible engine. A small magneto-machine with a Siemens armature was made to work a pump, or when turned by hand produced a current of electricity. Also a battery current in a Gramme ring in front of the poles of a Jamin magnet caused rotation of the ring and turned the heavy driving-wheel of the machine, and on removing the battery and turning the driving-wheel by hand, a current of electricity was produced which caused a piece of platinum wire to glow. Also a Tisley's hand dynamo-machine was employed either to heat a long piece of platinum wire or to drive another magneto-electric machine, so producing a secondary current capable of heating a considerable length of platinum wire.

(*To be continued.*)

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AT ALGIERS

Algiers, April 14

THE number of members enrolled for the present Congress is much larger than might have been expected when we consider the length of the journey. To a Parisian member this is such as would be experienced by a Londoner if our British Association met in Gibraltar. Yet more than 1500 names are on the list. Few of these are familiar to us. There appear to be an unusual number of doctors and professors of anatomy and physiology, and of civil engineers. We fear we must also confess that a great many people possessing little or no interest in science, who will not be present at a single sectional meeting, have joined the Association for the sake of seeing Algiers. The general character of the meeting appears to be that of a great excursion. There are only five days partially devoted to work; while the banquets, balls, fêtes, courses, and "fantasias" are rapidly multiplying.

On arriving in Paris we were told that the steamer which was to have conveyed us to Algiers had been requisitioned by the Government for the transport of troops to Tunis; but the Company determined at last to take both soldiers and *savants*, and the result was of course an overcrowded boat and excessive discomfort. With accommodation for fifty first-class passengers no less than one hundred and twenty-nine were crowded into the vessel, and had the voyage been anything but of the smoothest, it would have been most wretched. As it was it was bad enough; the food was insufficient in quantity and detestable in quality, and passengers were glad to find six feet of floor to sleep upon. In Algiers itself the hotels are quite full, and the salons will be used as dormitories.

The Congress will be opened to-day by the inaugural address of M. Chauveau, which is to be given in the theatre at three o'clock, after which the members will remain and resolve themselves into a general committee to discuss the creation of a sixteenth section relating to pedagogy; afterwards the secretaries of sections will meet to arrange their proceedings, and at 9 p.m. the members will be received by the Municipality at the Hotel de Ville. The general programme for the rest of the week is as follows:—

Friday, April 15.—Sectional meetings in the forenoon; general meeting at 2 p.m.; conference at half-past 8.

Saturday.—Sectional meetings in the morning; visit to the Algerian Exhibition in the afternoon; Arab *fête*, and a *soirée* given by the Municipality.

Sunday.—"Courses et fantasia."

Monday.—Sectional meetings in the morning; a procession through the town in the afternoon, and an Arab *fête* in the evening.

Tuesday.—Sectional meetings; general concluding meeting; in the evening a ball given by the Governor.

On *Wednesday* the excursions commence; they are both general and sectional, and the longest lasts for a fortnight.

A complete list of the papers to be read has not yet appeared, but in the list already published we do not note anything of special interest.

English science is represented by Dr. G. H. Gladstone and Mr. Siemens, who will both read papers.

The Association has presented to each member a work in two volumes entitled "Notices scientifiques, historiques, et economiques sur l'Alger et Algérie."

April 15

The theatre was well filled yesterday afternoon to hear the address of the president, M. Chauveau, who is Professor of Physiology in the Lyons Veterinary College. His discourse was of a far too technical character to be of interest to the majority of his audience, and dealt principally with the germ theory and Pasteur's theory of fermentation. It was read throughout without the least attempt at oratory, and it contained various political allusions which were much applauded. In the evening the members were entertained by the Municipality, and the town was illuminated. The real work of the Association commenced this morning, when the sections met at hours varying from eight to ten o'clock. We fear that the number of papers is small, and that the Association does not represent French science at all completely. In the Physical Section, for example, the names of only two authors of papers appear to-day—MM. Brillouin and Crova. At 9 a.m., when the section was announced to meet, no one was present. Shortly afterwards the secretary arrived, but an hour later the section had not met. The average number of the audience at the sections which had already met did not at this time exceed *ten*.

Among the more interesting papers announced for to-day are the following:—M. Marcheray, on Telephonic Communication in large towns; M. Tacchini, on the Observatories of Etna and of Chimona; M. Thoulet, on the Employment of the Microscope in Chemical and Physical Researches connected with Mineralogy; M. Prungrueber, on 300 Anthropological Observations on the Kabyles of the Djurdjura Mountains.

The Medical and Agronomical Sections have plenty of communications. The new section of Pedagogy has been established under the honorary presidency of M.

Godard, Director of the École Monge in Paris, who has brought twenty of the pupils with him. M. Fau, Attorney-General of Algiers, is the President, and two papers are announced for to-day:—One by M. Robert, on the Humanitarian and Pedagogic Ideas of Jean Comimiac (1572-1670); the other by M. Berdellé, on the Employment of Colours as a Means of Retaining in the Mind certain given Numbers.

An excellent geological map of Algiers to the scale of 1 in 800,000 has recently been completed, and the formation of it has led to the simultaneous observation of various facts connected with the physical geography of the Central Sahara. A map of the proposed interior sea of the Sahara has also been prepared, and the Trans-Sahara Railway is spoken of as more than a probability. But we very much doubt if this can ever be a success. If it connected flourishing towns or portions of territory in which great cities are ever likely to be established, as in the case of the new American railways, there would be some hope for it. As it is, however over-populated the world may become, there is no likelihood of the formation of settlements in the heart of an unhealthy tropical region.

G. F. RODWELL

MR. DARWIN ON VIVISECTION

FROM the *Times* we reproduce the following letter addressed by Mr. Darwin to Prof. Holmgren of Upsala, in answer to a request for an expression of his opinion on the question of the right to make experiments on living animals for scientific purposes—a question which is now being much discussed in Sweden:—

"Down, Beckenham, April 14, 1881

"Dear Sir,—In answer to your courteous letter of April 7 I have no objection to express my opinion with respect to the right of experimenting on living animals. I use this latter expression as more correct and comprehensive than that of vivisection. You are at liberty to make any use of this letter which you may think fit, but if published I should wish the whole to appear. I have all my life been a strong advocate for humanity to animals, and have done what I could in my writings to enforce this duty. Several years ago, when the agitation against physiologists commenced in England, it was asserted that inhumanity was here practised and useless suffering caused to animals; and I was led to think that it might be advisable to have an Act of Parliament on the subject. I then took an active part in trying to get a Bill passed, such as would have removed all just cause of complaint, and at the same time have left physiologists free to pursue their researches—a Bill very different from the Act which has since been passed. It is right to add that the investigation of the matter by a Royal Commission proved that the accusations made against our English physiologists were false. From all that I have heard however I fear that in some parts of Europe little regard is paid to the sufferings of animals, and if this be the case I should be glad to hear of legislation against inhumanity in any such country. On the other hand I know that physiology cannot possibly progress except by means of experiments on living animals, and I feel the deepest conviction that he who retards the progress of physiology commits a crime against mankind. Any one who remembers, as I can, the state of this science half a century ago must admit that it has made immense progress, and it is now progressing at an ever-increasing rate.

"What improvements in medical practice may be directly attributed to physiological research is a question which can be properly discussed only by those physiologists and medical practitioners who have studied the history of their subjects; but, as far as I can learn, the benefits are already great. However this may be, no one, unless he is grossly ignorant of what science has done for mankind, can entertain any doubt of the incalculable benefits which will hereafter be derived from physiology, not only by man, but by the lower animals. Look, for instance, at Pasteur's results in modifying the germs of the most malignant diseases, from which, as it so happens, animals will in the first place receive more relief than man. Let it be remembered how many lives and what a fearful amount of suffering have been saved by the knowledge gained of parasitic worms through the experiments of Virchow and others on living animals. In the future every one will be astonished at the ingratitude shown, at least in England, to these benefactors of mankind. As for myself, permit me to assure you that I honour, and shall always honour, every one who advances the noble science of physiology.

"Dear sir, yours faithfully,

"CHARLES DARWIN

"To Prof. Holmgren"

THE MAGNETIC SURVEY OF MISSOURI

IN the summer of 1878 the writer began a magnetic survey of the State of Missouri. The work of the first summer was confined to the north-east part of the State, and no points of interest were brought out. During the summer of 1879 the work was extended over the western half of the State, and it was made apparent that diversity of surface exerted a much more important influence than had been suspected. The lines of equal declination were found to bend very sharply upon entering the large valleys, and the needle showed a *tendency* to set at right angles to the valleys. This tendency seemed to be greatest when the general direction of the valley made an angle of 45° with the *normal* position of the needle, or roughly, when the valley runs north-east and south-west, or north-west and south-east. This tendency seems to be inappreciable when the valleys run north and south, or east and west.

In the report of 1878 (*Trans.* St. Louis Acad. of Sc., vol. iv. No. 1, p. 143) it was suggested that this might result from the bending of the stream-lines of the earth-current sheet, due to the greater conducting power of the moist valleys. In order to settle this point, further examination is necessary, and it is proposed to determine the earth-currents at a number of properly selected stations.

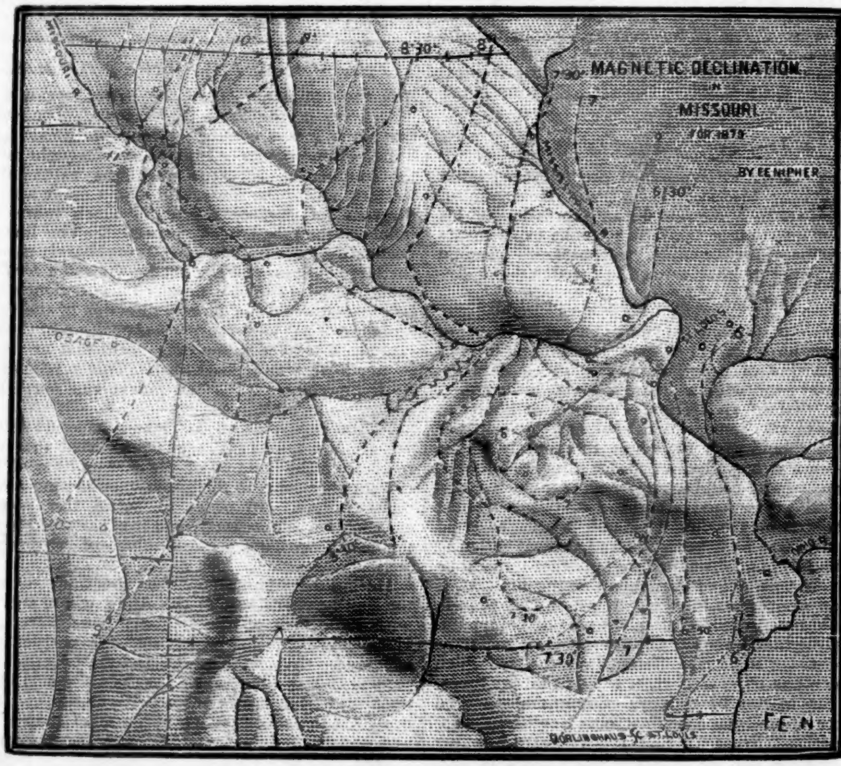
During the summer of 1880 the work extended over the south-eastern part of the State, where still more important flexures of the isogonic lines were discovered. Here, however, the position of the needle is probably affected by the iron deposits, and the effect of contour is studied to less advantage. At the close of 1880 observations had been made at forty-five stations. In order to bring out the effect of contour, a relief map of the State was constructed in wax, and was finally reproduced in plaster. In this work use was made of the profiles of all the railroads in the State, together with a list of over 300 elevations in the State, collected by Gannett. The isogonic lines, which were first drawn upon an ordinary map, in the usual manner, to represent the observations thus far made, were then copied upon the relief map. In doing this it became apparent at once that the forty-five stations were wholly inadequate, and that the isogonic lines thus drawn are probably deserving of about the same weight that a topographical map would deserve if constructed from elevations at these stations.

The Chart is made after an artotype, which will accompany the third annual report in No. 2, vol. iv. *Trans.* St. Louis Academy of Science. In the original map the horizontal scale is twenty miles to the inch, the elevations being exaggerated 200 times. This exaggeration was necessary in order to bring out the form in the photograph, since on a relief map, 150 feet square, the

greatest difference in elevation in the State would be represented by a vertical height of one inch. The horizontal scale of the cut is sixty-two miles to the inch.

The map represents only the grand outlines of surface as obtained from railroad profiles, and barometrical measurements. The dotted lines on the map are lines of equal variation of the magnetic needle: thus, on every part of the line marked 8°, the needle points 8° east of north, &c. These lines are drawn to represent the observations already made, and show in a general way the variation of the needle in the State. The map also shows that there is a marked relation between the direction of these lines and the contour of the surface. It cannot be said that it shows what this relation is, but it is probably due largely to the deflection of the stream lines of the earth current sheet, caused by unequal conducting power. This explanation necessitates the existence of looped areas in certain regions in the State, the existence of which is already indicated by the determinations. The loop in the 7° 30′ line, with its inclosed minimum, is probably complicated with the iron deposits in that region of the State.

Three stations in the Missouri valley have been inadvertently omitted in the cut. One of these (Corrollton) lies on the 8° 30′ line, a few miles north of the river. Another (Glasgow) lies on the river a little south of east from Corrollton. The third (Columbia) lies just east of the 8° line, and about south-east from Corrollton. A fourth station omitted, is nearly due east of the southern terminus of the 8° line, and just outside the 7° 30′ loop. The other stations, represented by the small circles, are shown on the cut, and an inspection of the map will show the weight to be given to different parts of these lines. At stations situated at points of abrupt curvature of the



lines, the observations have been repeated at various localities in the region, until it was clear that no minute local effects existed.

The value in the Iron Mountain region is the mean of many hundreds of determinations made with a solar compass by Pumpelly and Moore in 1872. This region is in the east part of the 7° 30′ loop. In the western iron-field, which is nearly coincident with the 7° oval, our observations were repeated at various points (the aim being to *avoid* iron deposits) without finding any local action.

In conducting the survey, a magnetometer belonging to Washington University was used, but the dip circle and declinometer were kindly furnished by Prof. J. E. Hilgard of the U.S. Coast and Geodetic Survey. Thus far the survey has been conducted wholly on private means, in which we have been aided by the railroad companies, and by citizens of St. Louis. A Bill providing for the completion of the survey is now before the Legislature of the State.

FRANCIS E. NIPHER

PRIMITIVE MARRIAGE CUSTOMS [1]

THE chief object of Mr. Fison's recently published memoir on Kamilaroi marriage, descent, and relationship, is "to trace the formation of the exogamous intermarrying divisions which have been found among so many savage and barbaric tribes of the present day," and to show that what Mr. Morgan calls the punaluan family, with the "Turanian" system of kinship, logically results from

[1] "Kamilaroi and Kurnai: Group Marriage, and Relationship, and Marriage by Elopement; also the Kurnai Tribe, their Customs in Peace and War." By Lorimer Fison, M.A., and A. W. Howitt, F.G.S. With an Introduction by Lewis H. Morgan, LL.D. (George Robertson: Melbourne, 1880.)

them. His coadjutor, Mr. Howitt, though he has had some interesting information to give about the Kurnai tribes of Gippsland, has had the same chief object; so that the work the two have produced is much more a polemic on behalf of Mr. Morgan than a record of new Australian facts. We must begin, then, by stating what Mr. Morgan's theories are (so far as the work before us is concerned with them), and indicating, and estimating the value of, the evidence on which they rest.

Mr. Morgan, having collected a great mass of facts concerning the terms in use between relations and connections throughout the world, and having found that those terms were, broadly speaking, divided into three orders, proceeded to spell out of the two earlier orders (the third consists of the modern terms of consanguinity and affinity) the whole of the early history of marriage and of the family. In what he has called the Malayan system of relationships, parent and child, grand-parent and grandchild, and brother and sister (or rather elder brother, younger brother, elder sister, younger sister, for there are no words for brother and sister) are the only terms in use; and one or other of these terms is used in addressing a person, according as the person addressed is of the speaker's generation or of the generation above, or of that below it. They are the terms always used when persons address one another, there being among those who use the system an invincible objection to the mention of their personal names. Mr. Morgan assumed that those terms were expressive of consanguinity and affinity; and conjectured that when first used they accurately described the relationships at the time existing, "as near as the parentage of children could be known." And it appeared to him that if there were a body of men and a body of women in the same tribe who all regarded each other as brothers and sisters, and all the men married all the women in a group, there would exist a marriage and family system which would explain the Malayan terms—the relationships arising out of which, so far as they were ascertainable, "as near as the parentage of children could be known," those terms would accurately express.

Accordingly, he framed the hypothesis that the first stage of marriage was the marriage in a group of men and women of the same blood calling themselves brothers and sisters. The family founded upon this kind of marriage he has named the consanguine family, and he regards it as the earliest form of the family. He does not say that such a system of marriage, or such a family system as he has supposed, has been found at any time anywhere; what he says is that this supposition of his explains the origin of the Malayan terms, and that nothing else can explain them. But does it explain them? It is at once obvious that there is one term, and that the most important of all, the use of which Mr. Morgan's hypothesis does not account for. Paternity may be doubtful—and if it were thought of at all in a group such as Mr. Morgan has conceived of, any man of the group might have as good a right as any other to be called father of any child born within it. But there can be no doubt about a man's relationship to his mother. In the case of mother and child the parentage is known with certainty, and therefore, on Mr. Morgan's hypothesis, a man should in the Malayan system have had only one mother. Now that system applies the term for mother to many women besides the actual mother—mother's sisters, father's sisters, uncle's wives, and so on, if not indeed to all women of the mother's generation. Here then the hypothesis breaks down; and the point at which we find it breaking down is really the only point at which it can be tested. The relationship between mother and child, too, which is confused or ignored in the Malayan system, is the one relationship of which it can be said with confidence that no system really founded on relationship could fail to recognise it. The explanation Mr. Morgan offers is that in the Malayan system the relationship of stepmother "is not discriminated," and there being no name for stepmother, stepmothers had to be called mothers, because "otherwise they would fall without the system." And he has what may be called a subsidiary hypothesis to account for there having been no discrimination between stepmother and mother. It is that the affiliation of children to the groups of men and of women to which they belonged would be so strong "that the distinction between relationships by blood and by affinity would not be recognised in every case." The fact of motherhood would be made little of, that is —there would be no discrimination between stepmother and mother—because the whole group would be, by a child, regarded as its mother. But this is equivalent to saying that, from the nature of the case, it was not to be expected that note should be taken of the relationships that could be known; and that is to abandon the hypothesis—as well as to deny us all chance of judging whether it is a good or a bad one. Possibly explanations of the failure of his hypothesis, such as Mr. Morgan suggests, might have some weight were he accounting for the Malayan terms as terms of address; but he takes them to denote actual relationships "as near as the parentage of children could be known." And no explanations can get over the fact that the Malayan terms are equally extensive in their application where, in the consanguine family, parentage would be known with certainty, and where it would not be known at all. The consanguine family is clearly a bad hypothesis. It might be thought it would hardly seem to anybody a plausible one; but Mr. Morgan always speaks of it as if it were among the best vouched of historical facts; and we are bound to say that Mr. Howitt believes in it as implicitly as Mr. Morgan.

To show the hypothesis of the consanguine family to be unstateable is to undermine Mr. Morgan's whole history of marriage and of the family. But Mr. Morgan has propounded a hypothesis as to the second form of marriage and the second form of the family, and as it is at this point that Mr. Fison (who does not quite believe in the consanguine family) lends him his advocacy, it is indispensable that we should give some account of it. Punaluan marriage, upon which was founded the punaluan family, was introduced by some reformatory movement, according to Mr. Morgan, to put a stop to the evils attendant upon brother and sister marriages. It existed in two forms. In one form of it a group of men, brothers or reputed brothers, had in common their wives who were not their sisters and not the sisters of each other; in the other form, a group of women, sisters or reputed sisters, lived in common with husbands who were not their brothers and not the brothers of each other. Punaluan marriage has not been observed at any time anywhere any more than the consanguine family; but Mr. Morgan believes that, in both its forms, it has existed everywhere, and probably during many ages. A correspondent wrote to Mr. Morgan stating that in the Sandwich Islands men whose wives were sisters and women whose husbands were brothers called each other punalua, which meant dear friend or intimate companion. And possibly drawing his bow at a venture, "the relationship," he said, "is rather amphibious. It arose from the fact that two or more brothers with their wives, or two or more sisters with their husbands, were inclined to possess each other in common." Whether conjecture or fact, this amounts to very little; but it was this which gave Mr. Morgan the suggestion of punaluan marriage. For proof of his hypothesis he again relied upon the terms he had collected—and at first upon its fitness to explain those same Malayan terms which, as we have seen, have more than enough to do to bear the weight of the consanguine family. In his latest work ("Ancient Society") he holds it to be proved by a nomenclature considerably

different from the Malayan—his second order of terms which he has named the Turanian system of relationships. He regards the terms in this system also as accurately describing, "as near as the parentage of children could be known," the relationships existing at the time when they came into use. It differs from the Malayan in including words for cousin, uncle and aunt, and nephew and niece—or words which Mr. Morgan has so translated. It will be found, however, that Mr. Morgan does not use the punaluan family in accounting for any one of the Turanian terms. Those of them which coincide, or partly coincide, with the Malayan terms he had already accounted for by the hypothesis of the consanguine family, and he does this over again; the others he accounts for, or tries to account for ("Ancient Society," pp. 442–445), by means of exogamy alone. His reasoning is exactly what it would have been had the punaluan family never occurred to him. Indeed it has been an embarrassment to him; he has had to keep it out of his reasonings. For the punaluan family is, *ex hypothesi*, in two forms, and neither form could, "as near as the parentage of children could be known," yield both the Turanian sense of father and the Turanian sense of mother. Where the husbands were punalua, Mr. Morgan's reasoning would make them all, though not brothers, fathers of children born within the group, and it would exclude their brothers from being considered fathers. But, in the Turanian system, a father's brothers are called fathers. Similarly where the wives were punalua, Mr. Morgan's reasoning would make them, though not sisters, all mothers of the children of each of them, and would exclude their sisters from being considered as mothers. But, in the Turanian system, a mother's sisters are called mothers. Mr. Morgan has not failed to see this, and he has actually again framed a subsidiary hypothesis to give his hypothesis of the punaluan family a chance of living. This is (see "Ancient Society," p. 445) that where a group of sisters married men who were not brothers, they also became the wives of all the brothers "own and collateral"—that is, all the brothers and one-half of the cousins, however far removed—of each of their husbands; and, similarly, that when a group of brothers married women who were not their sisters, they also became the husbands of all the sisters and one-half of the cousins of each of their wives. All that need be said of this subsidiary hypothesis is that it gives quite a new look to the punaluan family—and that the effect of it, like that of the secondary hypothesis formerly noticed, is to deny us all chance of judging whether the principal hypothesis is a good or a bad one. The justification offered for it is that "the system (the Turanian) treats all brothers as the husbands of each other's wives, and all sisters as the wives of each other's husbands, and as intermarried in a group"—but that is equivalent to saying that the system has taken no impression of the punaluan family, and gives no countenance to Mr. Morgan's hypothesis. As, apart from "the system," he finds nothing to say for it, it is difficult to see how any one can resist the conclusion that that hypothesis must be dismissed, and that it must be ranked among the wildest chimeras that have ever possessed the brain of a man of science.

Now, do Mr. Fison and Mr. Howitt give in any degree to Mr. Morgan's hypotheses the support of which they are in need? The answer must be no—and must be no even if we receive as facts the assumptions as to fact from which they set out. Mr. Howitt accepts both the consanguine family and the punaluan family, while Mr. Fison offers himself as the advocate of the latter only. But Mr. Howitt has nothing new to say for the consanguine family; he believes in it, and argues from it as if it were known historical fact—that is all; and so of it no more need be said. What then do his colleague and he find to say for the punaluan family? Literally, not a word. Mr. Howitt simply takes it for granted as he does the consanguine family. Mr. Fison, in beginning, undertakes to show that it results logically from his hypothesis —for it is no more than that—of "exogamous intermarrying divisions," but he does not attempt to do so. And, in fact, his "intermarrying divisions" are quite different from the punaluan family, and leave no need for it, and no room for it; that is, his hypothesis is different from and exclusive of Mr. Morgan's. In Mr. Fison's hypothesis, a group of men who are considered brothers and a group of women who are considered sisters—being the men and women of the same generation in two divisions which intermarry with each other, and only with each other—are by birth husbands and wives to each other; whereas, in the punaluan family, when the husbands are brothers the wives are not sisters—they are punalua; and when the wives are sisters the husbands are not brothers—they are punalua. Men who are brothers are restricted to women who are each other's sisters, on Mr. Fison's hypothesis; but, on Mr. Morgan's, men who are brothers marry women who, as a rule, are not each other's sisters. The marriage law shown in Mr. Fison's hypothesis would have to be given up before the punaluan family could have a chance of issuing out of the intermarrying divisions. Then, as Mr. Fison justly observes, his intermarrying divisions "would have precisely the reformatory effect" which Mr. Morgan attributes to the punaluan family—so that, given the divisions, the punaluan family would not be needed for reformatory purposes; and as Mr. Fison's view is that the totem clan grew up within his divisions, while their marriage law still subsisted, the punaluan family would not be needed to give birth to the clan (which Mr. Morgan says it has done). And, clearly, there would be no more room than need for it. It thus appears that, instead of supporting the hypothesis of the punaluan family, Mr. Fison has put it aside, and offers an improved hypothesis (suggested, no doubt, by Mr. Morgan's) in place of it. We have seen that he does not accept the consanguine family either. He does not, indeed, repudiate it. But to connect it with his intermarrying divisions seems to him so difficult that he thinks the one could have been changed into the other only through the intervention of "a higher power." He is not afraid of the ridicule to which he might be exposed were he to account for the first formation of the divisions by such a hypothesis; but he thinks it unnecessary to go behind them. We have now shown in what manner Mr. Fison supports Mr. Morgan—and we have shown that Mr. Morgan is in no position to give any support or countenance to him.

To show that the Turanian terms would result logically from his own hypothesis is what Mr. Fison has attempted. There are in a tribe two divisions which do not permit marriage within the division, and are restricted to intermarrying with one another. All the men in one division are the husbands of all the women of the same generation in the other; the wife does not come into the husband's division; and descent is reckoned through the mother. The group of men marries the group of women; and it is the group that is husband, the group that is wife, the group that is father, mother, son, or nephew; every person in it taking, however, all the relationships that arise to it. Such is the hypothesis. Seeing that the relationships are called group relationships, it might be thought that Mr. Fison considered the Turanian terms to have been, in the first instance, something other than terms of blood-relationship, say terms of address; but he denies that they are terms of address, and regards them as having been real relationships from the first. In what natural sense of relationship, however, a group—or the women in it other than the actual mother—can be mother of a child he does not tell us; and till he can make this plain, his theory must be held to be as untenable as the hypothesis of the consanguine family. As for his demonstrations (Q.E.D. at the end of each) of the Turanian

terms, we can scarcely pretend to follow them. The terms which are specially Turanian are laid down by him in definitions, and these definitions are used in the demonstrations—so that, so far as these terms are concerned, he seems to assume what he is going to prove. On p. 87 (Prop. 12) he proves that certain groups are cousins by the mere statement of three definitions. What is also odd is that, immediately after, he proves, by a process of reasoning, that the same groups are not cousins, but brothers and sisters-in-law. Similarly, he proves first that a group is another group's nephew, and then that it is its son-in-law. This brings us to say that the terms which Mr. Morgan has translated uncle, aunt, nephew, niece, and cousin, and which he regards as denoting relationships, according to Mr. Fison really mean father and mother-in-law, and brother and sister-in-law only, and express nothing except that a person is called father or mother, brother or sister, as the case may be, by a man or woman whom one is free to marry. How these could, with group marriage, be more than terms of address it puzzles us to see. What it is necessary to notice in these demonstrations, however—and nothing else is really necessary—is that while by hypothesis descent is reckoned through the mother—which must show that relationship had to some extent been the subject of thought—and "so far as descent is concerned, the father is a mere nonentity," they all proceed on the view that the father, who on the hypothesis would be in each particular case unknown, is as much a relative as the mother. Having said this, no more need be said of Mr. Fison's demonstrations. It should be added, however, that the terms in use among relatives in Australia are, so far as Mr. Fison can learn, in the main Malayan—and he has no theory to account for the Malayan terms. He knows nothing at all of the terms in use among the Kamilaroi. He has himself found the native terms "exasperatingly puzzling." Several terms may be used by the same people for one relationship, and, as he says, matters other than relationship appear to be taken into account. The ceremony of initiation, for example, affects the words by which a man will designate another, though, as Mr. Fison says, it "does not touch their relationship."

As to the hypothesis itself, an essential part of it (and indeed of Mr. Morgan's hypotheses too) is that, as regards the intercourse of the sexes, there should have been no mixing of generations—that only men and women of the same generation should have been husbands and wives. A generation, apart from particular families, can be defined only loosely, but for Mr. Fison's purposes it should be definable with some precision. At any rate, his theory requires that the elderly men should have been kept separate from the young women, and the young men from the old women. But what an assumption this is—especially to make primarily of Australian natives, of whom nothing is better known than that the elderly men monopolise the women, and especially the young ones, and that a young man (though much license is allowed) hardly ever gets a wife, unless it be an old one, except by running away with her. This assumption, experience being dead against it, is of itself enough to put out of the field the hypothesis of which it forms a part. The idea of intermarrying divisions with groups of husbands all brothers, and groups of wives all sisters, no doubt sprang out of the hypotheses of Mr. Morgan, but apart from Mr. Morgan, it has a history which must be told. Briefly, it was suggested by a traveller's mistake.

In 1853 the Rev. William Ridley, a Presbyterian clergyman of Sydney, published a statement as to the marriage rules of the Kamilaroi, which statement is now known, on Mr. Ridley's own authority, to have been essentially erroneous. Mr. Fison still treats it as entirely true, and treats all later and more correct information as if it gave facts of a later order. Mr. Ridley said that the Kamilaroi were divided into four castes of men and four of women, and that (with one exception) the men of a caste could marry only women of one other caste. Murri, feminine mata; kubbi, feminine kubbitha; kumbo, feminine butha; and ipai, feminine ipata, were the castes; and he said that a murri could marry a butha and no other woman, and that his children were not murri and butha, but ipai and ipata; and that, similarly, a kubbi could marry only an ipata, his children being kumbo and butha; and a kumbo only a mata, his children being kubbi and kubbitha; while an ipai, besides being free to marry any kubbitha, could marry any ipata not of his own family—his children, when he married a kubbitha, being murri and mata, and when he married an ipata, kumbo and butha. Mr. Ridley repeated this statement without change in 1855, and he told it in 1871 to Mr. Fison with this amount of change, that instead of castes he now spoke of classes (in unhappy imitation of Mr. Morgan), and of four classes, with men and women in each, instead of four classes of men and four of women; and that he described the marriage of ipai with ipata (that is with a woman of his own class) as an infringement of rule—changes that may fairly be ascribed to the initiative of Mr. Fison. Mr. Fison, putting aside the marriage of ipai with a woman of his own class as an irregularity, and idealising Mr. Ridley's statement, at once formed the hypothesis that all the men of one class originally were by birth the husbands of all the women of the same generation in the class with which they might intermarry. This, although he knew from Mr. Ridley that polygamy was largely practised among the Kamilaroi. Much licence was allowed; and the only word for spouse signified a person whom one is free to marry; and these two facts seemed to him to override Kamilaroi practice, and to prove that marriage had been communal, to begin. In the same year (1871), however, Mr. Ridley was again among the Kamilaroi, and sent to Mr. Fison a statement which should have shaken his faith in his hypothesis—both because of the new matter it contained, and because there were in it what he himself perceived to be errors of observation. Mr. Ridley has published several statements since, all containing obvious errors of observation or slips of memory, and it is impossible to receive even his latest statement as final. But observe what his latest statement is, and compare it with Mr. Fison's hypothesis. It is that the Kamilaroi are divided into totem clans (iguanas, paddy-melons, opossums, emus, blacksnakes, bandicoots); that every native has three names—a personal name (carefully concealed), a "class" name, and a totem name; that children take both the class name and the totem name through the mother; that the men and women of every class are free to marry one another, provided they are not of the same totem—and that, besides, murri may marry any butha, kubbi any ipata, kumbo any mata, and ipai any kubbitha. If his statements can be trusted, murri and butha, kubbi and ipata, kumbo and mata, and ipai and kubbitha, who are free to marry one another, are never of the same totem—so that all the marriages which certainly are permitted are marriages between persons of different totems. Mr. Ridley still leaves each class restricted from intermarrying with two others. So much of his original statement he has not yet found to be wrong. But the class name does not prevent marriage within the class. The notion that the Kamilaroi were in intermarrying or husband and wife "castes" was certainly erroneous. Is it likely then that the class-name is any bar to marriage outside the class? Is it not far more likely that there is still something for Mr. Ridley or some other inquirer to find out, and that, in the main, identity of totem is the only bar to marriage? We say in the main, because it is very likely that there are also regulations to prevent marriage between persons near in blood who are of different totems. Mr. Lance, who is a great authority with Mr. Fison, and who was Mr. Ridley's first informant, had got into his

head that the Kamilaroi were divided by their names into castes with the marriage law which Mr. Ridley first described, and, meeting with an ipai whose wife was an ipata, he regarded him as a daring transgressor of the customary rule. The man told him that he and his wife were free to marry because they were not of the same mudji (totem); and, thereupon, Mr. Lance (who evidently had never before heard of totems) told Mr. Ridley that the ipai were privileged above their neighbours in being free to marry women of their own class who were not of the same family with them; and Mr. Ridley told the world that they were the aristocratic caste among the Kamilaroi. (He has since stated that the murri are the aristocratic class.) This is the sort of observation we are questioning. Had Mr. Lance seen in operation a rule intended to prevent, say a man from marrying his own daughter, he might easily have magnified it into a rule prohibiting two whole "castes" from marrying. And in all probability it was something like this he did. It is the ludicrously wrong impression he had before he met the ipai aforesaid that Mr. Fison has taken for the basis of his hypothesis—but from even that to the hypothesis is a tremendous jump. And, after all, even if we overlook the inadmissible assumption which forms an essential part of the hypothesis, it appears not to be good for anything.

What have been called caste or class names appear, so far as the evidence goes at present, to be names merely, and to have no effect on the right of intermarriage. The system of naming is certainly very peculiar. The names alternate in successive generations. That is not in itself peculiar; but the same name is taken by all the sons, the same name by all the daughters. Thus ipata's children are the sons all kumbo, and the daughters all butha; and, again, butha's children are ipai and ipata. It is a pretty widely spread system. Mr. Howitt says that, as far as he knows, it prevails among all Australian tribes; but this is going a vast deal too far; and is calculated to undermine faith in Mr. Howitt's judgment, for it plainly does not prevail among the Kurnai whom he himself has described. His report shows nothing like castes or classes among them; the men, he says, are all called yeerung (emu-wren) by the women, and the women all djeetgun (superb-warbler) by the men; but this (whatever it may mean, and it may mean very little) does not divide the Kurnai into anything other than men and women. Mr. Fison has had from a number of correspondents statements which he takes to mean that among tribes other than the Kamilaroi which have this system of naming, there is no marriage between persons of the same name; but his correspondents are neither, as regards opportunity or observing power, above Mr. Lance; and Mr. Ridley's study of the Kamilaroi, imperfect as it has been, gives the only evidence that can be regarded as trustworthy. Mr. Fison has amended the list of marriages allowed among the Kamilaroi, given by Mr. Ridley, as he says, on later information; but anonymous information cannot be thought of much value on this matter as against the authority of Mr. Ridley. Mr. Fison is too easily satisfied with anything that seems to make for his view to be indly trusted in such a matter. We find him inferring from there being no marriage between blood-relations—which may mean totem clans—among people who have the class names that there is no marriage within the class. We find totem clans, too, reported to him as classes and ranked by him as classes; and "divisions," which probably mean totem clans, are also ranked by him as classes. On the other hand he candidly gives at least one case in which the class-names are said not to restrict marriage. He gives at the very beginning of his book a native legend of brothers and sisters having married at the first—a legend which both Mr. Morgan and he make much of. We are surprised, however, at his missing the true point of it. What it exhibits is not a movement to "intermarrying divisions" or classes, but to the establishment of totem clans. *These* are all the natives seem to have thought in need of explanation.

We should have been glad to notice Mr. Howitt's account of the Kurnai at some length, but we must be brief. The Kurnai have kinship through males and exogamy—that is, prohibition of marriage within the kindred; and as was to be expected in such a case, the kindreds form local tribes. He does not expressly tell us whether or not these clans or local tribes are distinguished by totems (which shows that he meant to be careful, and that his information was very far from being complete), but incidentally he lets out that they are. When a Kurnai young woman meets a young fellow who, being a stranger, looks as if he might make a husband for her, Do you eat kangaroo, opossum, blacksnake? is her first question after saluting him. Presumably the animal she names is her own totem. If the stranger may eat it he can marry her. As for his discovery of marriage by elopement, we have no doubt that it is (as a missionary friend of his, Mr. Bulmer, hinted to him it must be) a mere product of misconception. Young men among the Kurnai, he says, could get wives only by eloping with them on the proposal of the women. This may be; an Australian young man could scarcely ever get a wife except by running away with her. But how did the elderly men get their wives? He appears never to have asked that. But he is aware that there was a system of exchanges. The Kurnai are polygamous, and no doubt among them, as among other Australians, the elderly men had, by means of exchanges, nearly all the young women for wives. Mr. Howitt writes so candidly, and his account of the Kurnai is in many respects so interesting, that we should gladly have brought ourselves to think better of this discovery of his. But after reading Mr. Fison's most amazing account of the origin of marriage by elopement, we find ourselves shut up to holding that it is simply a big blunder. Nothing else could have elicited so preposterous an explanation. But such words as preposterous fall harshly on the ear, and we would part from our authors without unkindness. Their exertions to advance a growing science are truly commendable. If the result has been rather to mystify than to elucidate, there is but one more illustration of the way in which good intentions, industry, and ingenuity are wasted when men have started in the wrong track.

D. MacLennan

NOTES

The evening discourses at the meeting of the British Association at York will be delivered by Prof. Huxley and Mr. Spottiswoode. Mr. Huxley will speak of the "Rise and Progress of Palæontology" on Friday, September 2, and Mr. Spottiswoode "On the Electric Discharge, its Forms and its Functions," on Monday, September 5.

The Honorary Fellowship of the Royal College of Surgeons in Ireland was on Wednesday last week conferred on Prof. Helmholtz, and the Honorary Degree of LL.D. by the University of Dublin. On Monday night, at an ordinary meeting of the Royal Society of Edinburgh, Sir William Thomson in the chair, Prof. Helmholtz read a paper on "Electrolytic Conduction." There was a crowded attendance, and Prof. Helmholtz was warmly received.

On Monday the National Fisheries Exhibition, which has been organised at Norwich under the care of numerous public bodies, from the Board of Trade downwards, was opened by the Prince and Princess of Wales. The exhibition is divided into six classes, as follows:—1. Pisciculture and shell-fish culture; 2. Models, trawling gear, drifting gear, canvas and ropes, and inland fishing tackle; 3. Life-saving apparatus, lamps, fog-horns, signalling, &c., architectural plans for fish markets, fish-curing

establishments, fish vans, and fishermen's clothing; 4. Pictures, utilisation of condemned fish and fish refuse as a manure, and the cleansing of sewage-polluted streams; 5. Dried, salted, smoked, and tinned fish, shell fish, fish oils, manure, and disinfectants, aquatic flora and fauna, and birds which prey upon fish; 6. Loans. There is in the last class a fine collection of casts made by the late Frank Buckland, which has been lent by the authorities of South Kensington Museum. The exhibits of preserved specimens of aquatic, or rather fish-eating, birds is also highly interesting and instructive. In the class devoted to the culture of fish there are a large number of tanks, troughs, and the various appliances for hatching the ova of fish, for rearing the young fry, &c. There are also shown live fish in a series of aquaria, amongst which are specimens of nearly every variety of fresh-water fish indigenous to this country, the local fauna being especially complete. Next week we hope to give the address which Prof. Huxley, who is now one of the Inspectors of Fisheries, delivered in connection with the Exhibition.

FROM the *Sydney Morning Herald* we are glad to learn that a zoological station is at last to be established at Watson's Bay, under the direction of Baron N. de Miklouho-Maclay, on a small grant of land obtained from the Government. After a lengthy absence at the Melanesian Islands and in Queensland he has returned with the intention of remaining in Sydney until he can complete what he began in 1878, and see the zoological station firmly established. The land granted by the Government at Watson's Bay is situated near Camp Cove, and is about half an acre in extent. Upon this a cottage of five work-rooms will be erected, and this building will be for the use of naturalists who visit New South Wales for the purpose of studying the zoology and botany of Australia. The Government, understanding its scientific value, have given what assistance they can, and have promised that if 300*l*. be collected by private subscription a similar sum will be contributed towards the expense of erecting the building from the public revenue. The subscriptions up to the present amount to about 200*l*., and it is hoped there will be no difficulty in obtaining the balance which will be necessary for forming the zoological station on the small scale contemplated for a beginning. This station will be the first of the kind in the southern hemisphere, and will supply a serious want.

THE death is announced, at the end of February last, of Mr. Gerard Krefft, the Australian naturalist. Mr. Krefft was born in Brunswick, Germany, in 1830, and early conceived a taste for natural history. After spending some time in the United States he went to Melbourne in November, 1852. Mr. Krefft was selected to accompany the collecting expedition fitted out by the Victorian Government, 1858. Having succeeded the leader in command of the party he returned to Melbourne with a large collection of specimens and a well-filled portfolio, and was engaged by Prof. M'Coy as assistant in the Museum. He gave a report in full about the animals obtained and an account of the manners and habits of the aboriginals, illustrated by numerous sketches. He then resigned his position and returned to Germany. In 1859 he again left home for foreign lands, having obtained from the Hamburg firm, Messrs. Godeffroy and Son, a free passage for a trip round the world, and after a two months' sojourn in South Africa he took up his quarters in Sydney, being appointed secretary to the Australian Museum and assistant to the late Dr. Pittard, its curator. On the death of that gentleman Mr. Krefft succeeded to the vacant curatorship in 1861. During the latter years of his appointment he had a series of disagreements with the trustees of the Museum, which resulted in his leaving that institution, September, 1874. Mr. Krefft was probably the first man who thoroughly studied the reptiles of Australia. Mr. Krefft was a F.L.S., C.M.Z.S., and Member of various other learned societies.

AMONG Mr. Murray's forthcoming works are the following:—"The Formation of Vegetable Mould through the Action of Worms, with Observations on their Habits," by Charles Darwin, F.R.S.; "The Life and Letters of the late Sir Charles Lyell, Bart.," edited by his sister-in-law, Mrs. Lyell; "The Land of the Midnight Sun," being a narrative of summer and winter journeys through Sweden, Norway, Lapland, and Northern Finland, by Paul B. du Chaillu, of gorilla fame; also a second and revised edition of Mr. W. H. White's "Manual of Naval Architecture."

AT the meeting of the Sanitary Institute of Great Britain on April 13, Dr. Richardson read a paper of suggestions for the management of cases of small-pox and of other infectious diseases in the metropolis and large towns. Dr. Richardson maintains the thesis that there should be no aggregation of infectious cases in large central institutions, and describes the objections to such aggregation. He suggests further that the sanitary committee or authority in every parish should have al the special centres of infection in each of its districts thoroughly mapped out, and that it should know, on a calculation of cases occurring in quinquennial periods, what is the permanent accommodation required for its infectious sick. He urges that the required accommodation being known, the local authorities should keep ready at all times within the parish such necessary accommodation in small hospitals situated in different parts of the parish or locality. Dr. Richardson described what he thought should be the size, mode of construction, and position of such hospitals:—(*a*) That each hospital should not be larger than is sufficient to receive twenty-four persons at one time. (*b*) That each should be constructed on the separate system for the patients. (*c*) That each should be constructed of iron, so that it may at any time be absolutely purified by fire throughout all its structure. (*d*) That each should be placed on the upper storey of a building, forming in fact the top storey of one or more houses, so that it may be lighted and ventilated directly from its roof. (*e*) That all the air that passes out of the hospital when it is occupied by infectious persons should pass through fire. (*f*) That each patient should be carried into the hospital by a valved lift, which lift should pass through a shaft, so as to draw up air during its ascent, and which should, when required, be effective for flushing the hospital with air. Dr. Richardson entered into the subject of the organisation of these hospitals in respect to general supervision and nursing. Under this head he recommends—(*a*) That the general supervision should be in the hands of the Medical Officer of Health. (*b*) That the nursing, also under the supervision of the Medical Officer of Health, should be carried out by trained nurses, who might be educated to their work in the Union infirmaries. Lastly he suggested that the medical attendance should be conducted by a special staff of duly qualified medical men acting under the Medical Officer of Health and responsible to the local authority, by whom they should be approved and remunerated. An interesting discussion followed, which was adjourned to the 27th inst.

UNDER the auspices of the National Health Society the following Drawing-Room Lectures will be delivered at 23, Hertford Street, Mayfair (by kind permission of Mr. Charles Matthews), to commence at 4 o'clock p.m.:—Friday, April 22: Prof. Fleeming Jenkin, F.R.S., "Sanitary House Inspection"; Friday, April 29: Dr. Robert Farquharson, M.P. (formerly Medical Officer of Rugby School), "Health in Public Schools"; Friday, May 6: Mr. C. N. Cresswell, "Sanitary Relations of Local Self-Government"; Friday, May 13: Mr. Henry Power, M.B., F.R.C.S., "Care and Education of the Eye"; Friday, May 20: Dr. Siemens, F.R.S., LL.D., "Stoves and Grates"; Friday, May 27: Mr. Ernest Hart, M.R.C.S., "Recent Progress in Health Knowledge." Tickets may be obtained from the

secretary at the offices of the Society, 44, Berners Street, on Mondays and Fridays from 2 to 5, or will be forwarded by post on application. Patronesses: H.R.H. Princess Christian; H.R.H. Princess Louise, Marchioness of Lorne; H.R.H. Princess Mary Adelaide, Duchess of Teck. The president of the Society is His Grace the Duke of Westminster, and the Chairman of Council, Mr. Ernest Hart, M.R.C.S. The objects of the Society are to diffuse sanitary knowledge in every possible way, by the delivery of simple practical lectures on air, ventilation, food, and cookery, the prevention of the spread of infectious disease, and kindred subjects, at working men's clubs, mothers' meetings, and elsewhere, in all parts of London and the suburbs; by the circulation of sanitary tracts and papers; by encouraging the teaching of the laws of health in high schools and Board schools, by offering prizes, &c., to both teachers and pupils; and to secure open spaces for the healthy recreation of the people. Membership is constituted by the payment of 1*l.* 1*s.* annually; life membership by the single payment of 10*l.* 10*s.* Communications to be addressed to Miss Lankester, the Secretary, at the offices of the Society, 44, Berners Street, Oxford Street. It is evident that this Society has adopted effectual means to do a good and highly necessary work; it deserves the heartiest support from all who can in any way lend a helping hand.

Two Art Exhibitions were opened under the auspices of the Sunday Society on Easter Sunday, one at Whitechapel, the other in New Bond Street. That at the East-end consisted of a Loan Collection of Paintings, &c., organised by the Rev. S. A. Barnett, to which the Council on Education contributed largely from the National Collection at South Kensington. More than 2500 persons visited the Exhibition during the day, it being open from 2 till 9 p.m. The West-end Exhibition was the first Exhibition of the new Society of Painter-Etchers at the Hanover Gallery, and this was visited by about 578 members of the Sunday Society between the hours of 4 and 6.30 p.m. On Sunday, April 24, this Exhibition will be open to ticket-holders. Free tickets may be had by all who make a written application, inclosing a stamped and *addressed* envelope to Mr. Mark H. Judge, Hon. Sec. of the Sunday Society, 8, Park Place Villas, W.

THE second series of meetings organised by the Sunday Evening Association at the Neumeyer Hall was brought to a successful close on Easter Sunday by a lecture on "Lessing," by Mr. Moncure D. Conway. The hall was crowded, and at the close it was announced that the first annual meeting of the Association would be held in May, and that in the autumn the Committee intended to organise a series of meetings in the different Metropolitan suburbs, in order if possible to start local branches. The object of the Sunday Evening Association is to bring together all persons who, estimating highly the elevating influence of music and the sister arts, literature and science, desire by means of meetings on Sunday evenings to see them more fully identified with the religious life of the people. The subscription is 2*s.* 6*d.* per annum, which may be paid to the treasurer, Mr. Godfrey Shaen, 15, Upper Phillimore Gardens, W.

WE take the following from the *Electrician*:—The following correspondence over the telephone wires yesterday, says the Kansas City *Times*, is a further proof of the fact that no one but a bald-headed man could do without one:—"Hello, central!" "Hello!" "Connect me with the signal bureau." "All right—go ahead." "Hello, signal!" "Hello!" "Is it going to thaw to-day?" "Yes, there are indications." "How's the wind?" "Getting round to the south." "Do you think I can safely have my hair cut?" "Wait a minute until I consult the barometer, thermometer, and wind gauge." (Silence for half a minute.) "Hello!" "Hello!" "Yes, you can go ahead. There won't be any change to speak of for the next twelve hours. There is a cold wave moving up the Ohio River, and a snow-storm is reported at Cheyenne, but if I were you I'd take my chances on the hair-cutting." "All right—much obliged." "Good bye."

MR. PFOUNDES has reprinted, in a separate form, his short but interesting paper at the Anthropological Institute, on the "Japanese People: their Origin and the Race as it now exists."

THE usual meeting of delegates of the Sociétés Savantes takes place this week at the Sorbonne on a somewhat enlarged scale. It is the first time that members of Parisian societies will meet in combination with their provincial brethren.

A NEW fortnightly journal, *L'Electricien*, has appeared in Paris.

THE *Times'* Swiss correspondent states that the acclimatisation of the ibex in Switzerland would appear to be so far a success. The herd which was turned out some time ago in the Grisons is reported to have got through the winter without damage and as being at present in an excellent state of health.

A CORRESPONDENT of the *Daily News* points out that the exact time of the great Chios earthquake on the 3rd inst. was 1.50 p.m.

No. 2 of vol. iii. of the *American Antiquarian and Oriental Journal* (Chicago, Jameson and Morse) contains several contributions relating to American archæology, especially on the mound-builders. The editor, the Rev. S. D. Peet, has an article on the military architecture of the emblematic mound-builders, and there are various other contributions from various parts of the States bearing on the life and works of the prehistoric peoples of America. In the Oriental department various interesting points connected with Eastern antiquities are discussed.

PROF. CORNELIUS DOELTER of Grätz has safely returned from his journey to West Africa. He has brought home mineralogical and ethnographical collections.

THE Report of the West Kent Natural History Society speaks of its continued prosperity. It contains an interesting address by the President, in which he points out certain important bearings of recent researches on light. Speaking of the Blackheath holes, the President is inclined to think they are over the sites of pockets or pipes in the chalk, and that the cause of the subsidence is the drawing away of the subsoil by the action of water, the subterranean drainage produced by the pumping up of the water by the Kent Water Works at Deptford, drawing away sand and chalk in mechanical suspension.

THE following are among the papers in the *Transactions* of the Cumberland Association, part v.:—"Distribution of Boulders in West Cumberland," by J. D. Kendall, F.G.S.; "Soul-Cells and Cell-Souls," from the German of Haeckel, by the Rev. C. H. Parez; "The Influence of Geological Structure on Scenery," by Mr. Kendall; "The Local Museum and its Relation to the Natural History of the District," by Mr. James Arlosh; "The Moths of the District," by Mr. George Dawson; "The Character and Distribution of the Diatomaceæ," by Mr. B. Taylor; "Notes on Inglewood Forest," by Mr. John Jackson.

AN Electric Railway is being laid down in the grounds of the Crystal Palace.

THE Academy of Natural Sciences of Philadelphia are giving Spring courses of instruction in Invertebrate Palæontology and Mineralogy.

THE additions to the Zoological Society's Gardens during the past week include a Common Rhea (*Rhea americana*) from South America, presented by Mr. A. D. M. Stewart; a Mountain Ka Ka (*Nestor notabilis*) from New Zealand, presented by Dr. A. de Lantour, M.R.C.S.; an Undulated Grass Parrakeet (*Melopsittacus undulatus*) from Australia, two Californian Quails (*Callipepla californica*) from California, two Common Quails (*Coturnix communis*), a Greenfinch (*Ligurinus chloris*), a Goldfinch (*Carduelis elegans*), two Chaffinches (*Fringilla cœlebs*), two Common Crossbills (*Loxia curvirostra*), a Common Lapwing (*Vanellus cristatus*), British, a Barred Dove (*Geopelia striata*), a Nutmeg Bird (*Munia undulata*) from India, two Rufous-necked Weaver Birds (*Hyphantornis textor*) from West Africa, two Mecca Pigeons (*Columba œnas*, var.), from Tunis, presented by Mr. H. H. Johnston; a Green Turtle (*Chelone viridis*) from West Indies, presented by Mr. J. C. Robinson, R.M.S. *Dunrobin Castle;* a Common Viper (*Viper berus*), two Common Snakes (*Tropidonotus natrix*), British, presented by Mr. J. Poyer Poyer; a Red-faced Saki (*Brachyurus rubicundus*), a Horrid Rattlesnake (*Crotalus horridus*) from South America, a Brown Bear (*Ursus arctos*) from Spain, a Great Kangaroo (*Macropus giganteus*), two Ursine Dasyures (*Dasyurus ursinus*), three Vulpine Phalangers (*Phalangista vulpina*) from Australia, deposited; a Beisa Antelope (*Oryx beisa*), a Banded Ichneumon (*Herpestes fasciatus*), a Squirrel-like Phalanger (*Phalangista sciureus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE SOLAR PARALLAX.—M. Puiseux, in a communication to the Academy of Sciences of Paris, discusses the numerous micrometrical measures made during the last transit of Venus by MM. Mouchez and Turquet at St. Paul's, and MM. Fleuriais and Bellanger at Pekin. If these observations had possessed a high degree of precision he considers that they would have furnished a very exact value of the solar parallax, but unfortunately, so far at least as regards the measures at St. Paul's Island, the conditions were extremely unfavourable; indeed in a note which follows M. Puiseux's communication Admiral Mouchez remarks that the equatorials provided for that station had no special appliances for this class of observation, and worse still, "les observations ont été faites exactement au moment du passage du centre d'un violent cyclone, pendant la courte éclaircie qui accompagne la plus grande dépression barométrique." The instruments in fact were more particularly adapted to proposed observations of contacts, and were very weakly mounted; oscillations were occasioned by the violent wind, so that the practised observers had no confidence in their results. Notwithstanding these circumstances M. Puiseux has discussed the measures, and from the combination which he regards as the most favourable, where 81 observations that appear affected with considerable errors are rejected, leaving 312 measures for calculation, he deduces for the value of solar parallax 9″·05: the mean value of the corresponding residuals is 0″·78, and the extreme residuals −1″·98 and +2″·15. Considering that under such disadvantageous conditions the observations accord passably, M. Puiseux thinks there are reasonable grounds to expect that with firmly-mounted instruments micrometrical measures may be obtained at the approaching transit in 1882, which will furnish a pretty exact value of the sun's parallax.

THE DOUBLE-STAR HERSCHEL 3945.—The double-star to which Mr. Birmingham drew attention in NATURE last week on the score of contrasted colours of the components and variability of the principal star has a longer history than is noted in his letter. It is found as a single star of sixth magnitude in Bode's Catalogue, from his own observation, and is Canis Majoris 164. Lalande observed both components on March 2, 1798, magnitude 5 and 7. On January 23, 1835, Sir John Herschel, observing at the Cape, calls them 7 and 8, "large star orange: small, pale blue": and on January 31, 1837, he estimated the magnitudes the same: "large star, very high yellow; small, contrasted blue"; these observations occur in Sweeps 532 and 768. Amongst his micrometrical measures we find for the epoch 1837·153 magnitudes 6½ and 7, and for 1837·301 magnitudes 6½ and 8, with the note "Orange and green, fine contrast of colours." Next we have three meridian observations of the principal star by Argelander in vol. vi. of the "Bonn Observations," on January 26 and March 13 and 14, 1854, magnitudes noted, 5·5, 4·5, and 5·0, and one observation of the companion on March 23 in the preceding year, when it was estimated 7·5. In Heis and Argelander the naked-eye estimate is 5 m. The components are separately noted in Gould's *Uranometria Argentina* A. 5¼ red, B. 7. The star does not occur in D'Agelet, Taylor, or in Argelander's Southern Zones. The mean place for the beginning of the present year is in R.A. 7h. 11m. 31·80s., N.P.D. 113° 6′ 19″·4.

THE TOTAL SOLAR ECLIPSE OF 1878.—In one of the handsomely-executed volumes which issue from the Government Printing Office at Washington, the U.S. Naval Observatory has published the detailed reports of the various expeditions organised for the observation of the total eclipse of the sun on July 29, 1878, which possess a high degree of interest. A large number of wood-engravings and lithographic plates accompany the reports. There is also appended a brief account of the observations made in California during the total solar eclipse of January 11, 1880.

THE EARTHQUAKE OF NOVEMBER 28, 1880, IN SCOTLAND AND IRELAND[1]

THE data on which the paper has been founded have been collected from upwards of fifty stations, and special reliance may be placed on the results, as a large proportion of these stations were lighthouses, in each of which at the time of the occurrence there was a keeper *on watch*, the earthquake having occurred after sunset at a time when the lamps were lighted.

The paper at the outset gave the effects and nature of the shock experienced by various observers at those lighthouse stations where the disturbance was felt.

The data acquired were then discussed, and the following are the general conclusions arrived at:—

1. That the earthquake occurred in November, a month in which many of the British earthquakes are recorded as having happened.

2. That it occurred after a wet and stormy period, which had been preceded by an unusually dry summer and spring; that there was a widespread thunderstorm at the time, and that the barometer was rising slowly over the greater part of the west of Scotland; the average height of the barometer at the lighthouse stations at which the earthquake was felt being at 9 a.m. 29·4 inches, and at 9 p.m. 29·5 inches. The thermometer at 9 a.m. averaged 50° F., and at 9 p.m. 48° F.

3. That the seismal area was about 19,000 square geographical miles, the shock having been felt as far north as the Butt of Lewis, as far south as Armagh in Ireland, as far east as Blair Athole, and as far west as Barra Head Lighthouse, though how much farther it was propagated into the Atlantic it is impossible to say.

4. That the range of the earthquake or distance to which the wave was propagated was greater over the sea than over the land.

5. That the earthquake was not a simultaneous shake over the disturbed area, but was produced by a wave propagated from a centre.

6. That the undulation seems to have been chiefly of an "up and down" character like a wave of the sea, and that calculating the "breadth" from the mean velocity of transit and the minimum duration of the shock, the wave appears to have been fully 1100 feet "broad."

7. That the observations warrant the assumption that a spot near Phladda Lighthouse (north-east of Colonsay) was the source, and calculating the velocities of transit with a point 13 miles south-south-west of Phladda Lighthouse as a centre, it appears that the wave travelled with a greater velocity over the sea-basin than over the land, probably due to the fact that over the sea there was a thinner and lighter crust to throw into vibration; the average velocity on sea journeys being 6·74 geographical miles per minute, and the average velocity on land journeys 4·65 miles per minute, the mean of the whole being about 5¾ miles per minute.

8. That the source of the earthquake lay at or near the great

[1] By Charles Alex. Stevenson, B.Sc., Edinburgh, communicated to the Royal Society of Edinburgh by Prof. Geikie, F.R.S., March 21, 1881.

fracture of the earth's crust which runs in a south-westerly direction from Inverness.

9. That all the observers who heard the noise agree in stating that it was a "rumbling" sound.

10. That of the fourteen observers within 38 miles of the source who felt the shock, thirteen of them mention having heard the rumbling noise, and none of the other observers in Scotland mention noise as an accompaniment of the earthquake, and hence that the noise was confined chiefly, if not entirely, to places situated near the source.

11. That the stations where the noise was heard were for the most part situated on hard dense rocks, with little or no soil near them.

12. That the average duration of the disturbance was 4·4 seconds for observers situated within the sound area.

13. That of twenty-two lighthouse observers between Cape Wrath and the Mull of Galloway who were situated on the older formations (Laurentian, Cambrian, and metamorphosed Lower Silurian) eleven felt the shock, whilst of thirteen observers situated on newer rocks it made itself known only to two of them, and that the earthquake was therefore more generally felt on the older rocks of Scotland.

14. That stations situated near one another and on the same formation did not necessarily both receive the shock, and that faults and trap dykes did not seem to affect the passage or intensity of the wave in any way.

15. That the observations of time at Armagh, Belfast, and Omagh show that the shocks at these places were most probably propagated direct from Phladda in Scotland, and that the severity of the shock and the "rumbling" noises heard in and around Leterkenny were probably due to a second and local source of disturbance generated by the arrival of the shock from Phladda.

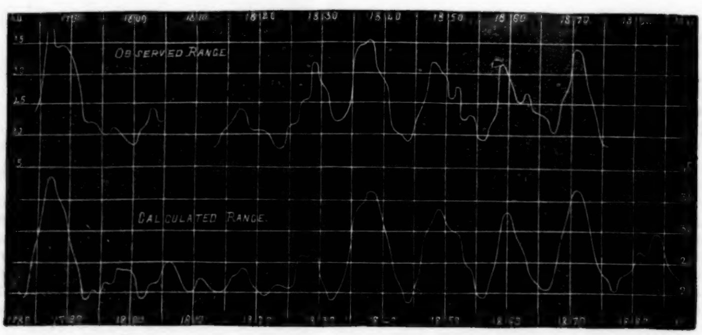
MAGNETIC DECLINATION [1]

1. IT is well known that Prof. Rudolph Wolf has endeavoured to render observations of sun-spots made at different times, and by different observers, comparable with each other, and has thus formed a list exhibiting approximately the relative sun-spot activity for each year. This list extends back into the seventeenth century, and is unquestionably of much value. Nevertheless it must be borne in mind that we possess no sun-spot data sufficiently accurate for a discussion in a complete manner of questions relating to solar periodicity before the time when Schwabe had finally matured his system of solar observations, which was not until the year 1832.

We have however a much longer series of the diurnal ranges of magnetic declination. Now these are already well known to follow very closely all the variations of sun-spot frequency, being greatest when there are most, and least when there are fewest spots; and it may even be imagined that such ranges give us a better estimate of true solar activity than that which can be derived from the direct measurement of spotted areas.

The long-period inequalities of the diurnal range of magnetic declination are thus, we may imagine, precisely those of solar activity, so that to analyse the former is probably equivalent to analysing the latter.

2. Our method of analysis is not new. The system pursued by us is in fact that which has been pursued by Baxendell, and probably other astronomers, with observations of variable stars, and it has already been applied by one of us in a preliminary manner to magnetic declination ranges (*Proc.* Lit. and Phil. Society, Manchester, February 24, 1880).



3. The observations at our disposal are those which have been used by Prof. Elias Loomis in his comparison of the mean daily range of the magnetic declination with the extent of the black spots on the surface of the sun (*American Journal of Science and Arts*, vol. l., No. cxlix.). These observations are recorded as monthly means of diurnal declination range, and we found it necessary to multiply each by a certain factor, firstly, on account of the well-known annual inequality of declination range, and secondly, to bring them all to the standard of the Prague observations. We have applied for this latter purpose precisely the same corrections as those made by Prof. Loomis.

4. The result of an analysis of these observations has been to indicate the existence of three inequalities: two dominant ones with periods of about 10½ and 12 years, and a subsidiary one with a period of about 16¼ years. By these means we have been enabled to reproduce the observed annual values of declination range with an average difference of 39″. The amount of agreement between the observed and calculated values will be seen from a diagram which accompanies this note. We are however of opinion that the series of observed values at present obtainable is too short to render this analysis a very accurate one. It will certainly not bear carrying back forty or fifty years beyond its starting-point, which was in 1784, and it would be very hazardous to carry it forward any considerable length into the future. We may however mention that our calculations indicate a maximum of declination range about 1884, but not so pronounced a maximum as that of 1871.

5. During our analysis an observation was made by us which we think worthy of record.

It is a well-known fact that the so-called eleven-yearly oscillations of declination range are at certain times large, and at other times small. Thus, for instance, they have been large for the last forty years, but they were small about the earlier part of the present century. It is clear to us from an inspection of the observations that a series of large oscillations is accompanied with an exaltation of the base line, or line denoting average efficiency, while a series of small oscillations is accompanied with a depression of the same. The result is a long-period curve of the base line, the beat period, so to speak, of the eleven-yearly inequality.

Now a phenomenon precisely similar occurs in connection with shorter periods. If we take inequalities having a period of three or four months we find that such are alternately well developed or of large range, and badly developed, or of small range; and that a large range of such is accompanied with an exaltation of the base line or line of average efficiency, while a small range is accompanied with a depression of the same. The result is a curve of the base line, of which the period is, roughly speaking, eleven years. May we not therefore imagine that the so-called eleven-yearly period, or, to speak more correctly, the

[1] "Note on an Attempt to Analyse the Recorded Diurnal Ranges of Magnetic Declination." By Balfour Stewart, M.A., LL.D., F.R.S., Professor of Natural Philosophy at the Owens College, and William Dodgson. Read at the Manchester Literary and Philosophical Society, March 8.

ten and a half and twelve-yearly periods into which the eleven-yearly period may perhaps be analysed, may be in reality beat periods for shorter disturbances? Is it not therefore possible that a study of these shorter periods may give us information regarding the nature of the eleven-yearly period, whether for sun-spots or declination ranges, which the small series of actual observations is incompetent to afford?

We beg to take this opportunity of thanking Mr. William Stroud for the help he has given us in this investigation.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At Trinity College the following distinguished graduates of the College have been elected Honorary Fellows:—Lord Rayleigh, M.A., F.R.S., Professor of Experimental Physics; Mr. Henry Sidgwick, M.A., Prælector in Moral and Political Philosophy, the author of "The Method of Ethics"; Mr. Edward Herbert Bunbury, M.A., author of "A History of Ancient Geography," &c.; and Mr. William Henry Waddington, B.A., Member of the French Institute, late President of the Council, and Minister of Foreign Affairs in France.

The Adams Prize is to be given in 1883 for a general investigation of the action upon each other of two closed vortices in a perfect incompressible fluid. In particular it is suggested that the case of two linked vortices should be fully discussed, with the view of determining (1) whether any steady motion is possible, and (2) whether any motion can occur in which there are periodical changes in the forms and dimensions of the vortices. Each essay should be accompanied by a full and careful abstract pointing out the parts which the author considers to be new, and indicating the parts which are to be regarded as of more importance than the rest. The competition is open to all graduates of Cambridge; essays must be sent in on or before December 16, 1882. Essays must not be written in the candidate's own hand. The successful candidate will receive about 170*l*. He must print the essay at his own expense. The examiners are the Vice-Chancellor, and Messrs. A. Freeman, W. H. Besant, and E. J. Routh.

VICTORIA UNIVERSITY.—The following summary of draft regulations on degrees, examinations, and courses of study has been issued:—1. These regulations are, with the exception of certain general proposals with reference to University Matriculation, confined to the subjects of Degrees, Examinations, and Courses of Study in the Faculties of Arts and Science. 2. According to the proposals in the Report any certified student of a College incorporated in the University may matriculate at certain times in the year, the definition of College studentship being left to the College or Colleges, subject in each case to the approval of the University. No University examinations leading to a degree will be open to any persons who are not matriculated students. 3. According to the proposals in the Report there are to be two distinct Faculties of Arts and of Science. The degrees in these faculties are to be those of B.A. and M.A., B.Sc. and M.Sc., and a Doctorate common to the two Faculties and varying as a Doctorate of Literature, of Philosophy, and of Science. 4. In consonance with a main principle of the University Charter, the degrees of B.A. or of B.Sc. are to be conferred upon students who have passed certain prescribed University examinations, and who have attended certain prescribed University courses of study in a College of the University. 5. In the examinations for the degrees of B.A. and B.Sc., and in the privileges conferred by these degrees, a distinction is to be drawn between the Ordinary B.A. or B.Sc. degree, and the B.A. or B.Sc. degree with Honours. 6. The regular period of study required of candidates for the degrees of B.A. and B.Sc. is to be three years, of which two shall be after the date of their passing the Preliminary Examination (see § 7 of this summary); but students who have passed the Preliminary Examination (see § 7 of this summary) next in date after their matriculation, and have been placed in the first division of the list of successful candidates, shall be allowed to proceed to their degree in two years. 7. All candidates for the degrees of B.A. or B.Sc. are required to pass a general examination called the Preliminary Examination, and to present themselves for this examination not later than two years from the date of their matriculation. Regular first year courses of study are arranged as preparatory for this Examination, to be taken by all students except those who pass it immediately after matriculation (see § 6 above), or who go through the first year's course of one of the Honours Schools approved by the University. 8. The subjects of the Preliminary Examination are arranged in two groups (A and B), in one of which every candidate must pass. The essential difference between the two groups is that in A, Latin and Greek are compulsory, but that a choice is given between four subjects, including two modern languages and two elementary sciences; while in B a choice is given between the alternatives of two languages (ancient or modern) and one elementary science, or two sciences and one language (ancient or modern). In B the requirements in modern languages and mathematics are rather greater than in A. 9. The other examinations for the degrees of B.A. and B.Sc. will be open to such students only as have passed the Preliminary Examination, and as have attended the prescribed University courses of study in a College of the University. These further examinations will differ for students intending to present themselves for an Ordinary B.A. degree and for those desirous of a B.A. degree with Honours. 10. The degree of B.A. with Honours is to imply that a student has attended, during three years, prescribed courses of study (approved by the University) in a distinct branch of learning or science forming the subject of one of the Honours Schools of the Faculties of Arts and Science, and that he has passed a prescribed examination in such Honours School after attending its third year's course. The Honours Schools recommended in the Report for immediate establishment in the University are those of (1) Classics, (2) English, (3) History, (4) Philosophy, (5) Mathematics, (6) Engineering, (7) Chemistry, (8) Zoology, (9) Physiology, and (10) Geology, Mineralogy, and Palæontology. For all of these Honours Schools the Owens College is prepared to supply classes meeting the proposed requirements of the several Schools. 11. The Ordinary degree of B.A. or B.Sc. is to imply that a student has attended, during at least two years, prescribed courses of study (approved of by the University) forming a connected whole, and that he has passed an examination corresponding to the earlier year's course, to be called the Intermediate Examination, and an examination corresponding to the later year's course, to be called the Final Examination. 12. The courses of study, and the corresponding examinations, prescribed for the Ordinary degrees of B.A. and B.Sc., and open to the choice of candidates who have passed either group of the Preliminary Examination, vary according to the predominance in each course (with its examinations) of one branch of learning or science. This predominance is not however such as to warrant the maintenance of the designations given (for convenience' sake) in the Draft Regulations of "mainly Classical, Historical, English, Philosophical, Mathematical, Engineering, Experimental Science, and Biological." Candidates for an Ordinary B.A. or B.Sc. degree may choose any of these groups, but must go through the whole two years' course, and pass both the examinations of the group chosen. The examinations and classes however largely coincide in particular portions of the several groups. 13. With a view to encourage more advanced study in special branches of learning or science in students whose bent has been determined, or whose capabilities have been developed, at a later stage of their University career, students who have passed the Final Examination for an Ordinary B.A. or B.Sc. degree, are to be allowed to present themselves for examination for a B.A. or B.Sc. degree with Honours, after attending the third, or second and third, year's Honours Course, only in one of the Honours Schools. 14. The degree of Master of Arts or of Science is to be conferred upon Bachelors of Arts of three years' standing, after not less than six years from the date of their matriculation. B.A.s who have graduated with Honours are not to be required to pass any further examination for the M.A. degree; those who have taken the Ordinary B.A. or B.Sc. degree are to be required to pass an examination in some portion of one of the Honours Schools Examinations. 15. The Doctor's degree in the Faculties of Arts or Science is to be conferred upon M.A.s or M.Sc.s who have furnished evidence of special research or learning, to be supplemented when desirable by an examination test.

SCIENTIFIC SERIALS

Bulletin de l'Académie Impériale des Sciences de St. Petersbourg, t. xxvii., No. 1, February, 1881.—On the results of experiments on the resistance of the air and their application to the solution of problems of firing, by M. Mayevski.—On variations of the fur and on the geographical distribution of the sea-otter (*Enhydris marina*), by M. Brandt.—On the integration of partial equations of the first order with several variables whose co-efficients are constant,

by M. Alexéeff.—On the rotation of Jupiter, by M. Kortazzi.—Crystals of beryl from a part of the Southern Oural, by M. Kokscharow.—On the formation of some nitrated derivatives of some hydrocarbons of the fatty series by direct action of nitric acid, by M. Konowalof.—On the variability of forms of *Lubomirskia Baicalensis*, and on the distribution of sponges of Lake Baikal, by M. Dybowski.—On universal time, and on the choice for this purpose of a prime meridian, by M. Struve.—Anatomy of the lactiferous glands during the period of lactation, by M. Saefftigen.—On the spectroscopy of hydrogen, by M. Hasselberg.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xiv. fasc. 4 (February 24).—On a method of finding with the microscope the adulterations of the more common varieties of farina, by Dr. Cattared.—Experimental researches with the Crookes' apparatus, by Prof. Ferrini.—On a quadratic Cremonian correspondence between the elements of two fundamental forms of the fourth species, or ruled spaces, by S. Aschieri.—Considerations on new species of partial blindness in Arachnida, by Prof. Pavesi.

Fasc. 5.—Materials to serve for the study of *Peronospora viticola*, by Count Trevisan.—On primary and secondary psoïtis, by Prof. Sangalli.—The sanitary administration in Spain, by Dr. Quechi.—Determination of the maximum moments due to weights linked on a supported beam, by Prof. Clericetti.—On an abnormal case of fructification in Florideæ, by S. Ardissone.

Revue Internationale des Sciences, February, 1881.—Prof. Vulpian, physiological study of poisons, vii. Curare (end).—Prof. R. Lankester, embryology and classification of animals.—Fernand Lataste, a few more words on the fecundation of the urodele batrachians.—Notices of learned societies.—Belgian Academy (abstract of Van Bambeke's paper on the formation of the embryonic lamellæ and the notochord in the urodela).—Paris Academy: on the appointment of M. Bouley to the Chair of Comparative Pathology at the Natural History Museum, Paris.

Journal de Physique, March.—On the division of instantaneous currents (continued), by M. Brillouin.—On the psychrometer, by M. Angot.—New tourmaline pincer, by M. Bertin.—Constitution of the flame of the Bunsen lamp, and some modifications in the construction of this lamp, by M. Terquem.—On some experiments in acoustics, by M. Neyreneuf.

Atti della R. Accademia dei Lincei, vol. v. fasc. 7 (March 6).—On solar observations at the Royal Observatory of the Roman College in 1880, by P. Tacchini.—Observations of comets and planets at the same college with the Merz equatorial, during 1880, by the same.—M. Janssen's solar photographs taken at Meudon Observatory, by the same.—Thermal laws of the exciting spark of condensers, by E. Villari.—On sodio-ammoniacal trimolybdate, by F. Mauro.—Studies on rotatory power, by R. Inasinio.—On some compounds of the pyrolic series, by L. G. Ciamician.—On the electrophorus, by G. Govi.—On pathological bases, by F. Selmi.—On the causes of distinctness in solar photographs, by S. Respighi.—On experiments made at the Observatory of Campidoglio for determination of gravity, by the same.

Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden (1880).—A modern investigation of the flora of Saxony, by Prof. Drude.—On the Pycnodontidæ, especially the genus Gyrodus, by Prof. Vetter.—The Nudibranchia of the sea, by Herr Blaschka.—On the determination of fixed points of normal mercury thermometers and the measurement of temperatures, by Prof. Neubert.—On various finds in the neighbourhood of Dresden, by Dr. Caro.—Hydroid medusæ or Craspedotes, by Herr Blaschka.—Progress of geological researches in North America, by Dr. Geinitz.—On plant-remains from the Tertiary formations of Liebolitz and Putschein, by Herr Engelhardt.—Observations on the growth of the leaf of *Victoria regia*, Lindl., in the Dresden Botanical Gardens in 1880, by Prof. Drude.—The Slav and German immigration into Saxony, by Prof. Meitzen.—The urn-field of Persia, by Herr Wiechel.

Archives des Sciences Physiques et Naturelles, No. 3, March 15. Swiss geological review for 1880 (continued) by M. Favre.—Considerations on the study of phyllotaxy, by M. de Candolle.—Notice of researches by Drs. Tenchini and Staurenghi, on the anatomy of the human cerebellum.

Rivista Scientifico-Industriale, No. 5, March 15.—On Reese's fusing disk, by Prof. Bombicci.—Volta's pile rendered constant and depolarised, by Count Mocenigo.

SOCIETIES AND ACADEMIES

London

Royal Society, March 31.—On the coefficients of expansion of the iodide of lead, and of an alloy of iodide of lead with iodide of silver, by G. F. Rodwell, Science Master in Marlborough College.

The iodide of lead was examined by the special means described by the author in former communications to the Society, and was found to possess three coefficients of expansion. Between 0° and 205° C. the coefficient of cubical expansion for 1° C. is ·0000 7614, increasing to ·00008307 between 205° and 253° C.

Between 253° and 265° C. the mass expanded very rapidly, with a coefficient nearly eight times greater than the preceding, viz. ·0006378. After the subsidence of this rapid expansion the coefficient became ·000180. The volumes of the iodide between 0° and the fusing point (383° C.) are given, and are shown in a curve-table.

Iodide of lead was fused with iodide of silver in such proportions as to form an alloy containing one molecule of each constituent, viz. PbI_2, AgI. This contains 66·20 per cent. of iodide of lead, and 33·80 per cent. of iodide of silver. The melting-point of the alloy was found to be 350° C., the specific gravity 5·912. On heating it was found to expand under a very low coefficient between 0° and 118° C.; then it neither expanded nor contracted while heated through 6° C.; at 124° C. it commenced to contract, and underwent between 124° and 139° C. as much contraction as iodide of silver itself; again it was stationary for 5° C., and at 144° C. it began to expand again, with a much higher coefficient than it possessed between 0° and 118° C.

The following are some noticeable points about the alloy:—

1. It possesses similar densities at three different temperatures.
2. Although it contains only 33·8 per cent. of iodide of silver it contracts as much during heating as the iodide of silver itself.
3. While the iodide of silver commences to contract at 142° C., and terminates at 145°·5 C., the alloy commences to contract 18° C. lower, and terminates its contraction 6°·5 C. lower.
4. The harsh sounds emitted during the cooling of the alloy, and the tremors simultaneously propagated through its mass, prove that violent molecular agitation is taking place while the iodide of silver is passing from the amorphous plastic condition, into the brittle crystalline condition within the mass of, or surrounded by the molecules of, the iodide of lead.
5. The fusing-point of the alloy is 33° lower than that of the iodide of lead, which constitutes two-thirds of its weight, and 177° lower than that of the iodide of silver, which constitutes one-third of its weight.
6. And if this is due to the fact that similar particles of matter attract each other more powerfully than dissimilar, and hence that when the particles of two bodies are mutually diffused the attraction becomes less than that of the molecules of either one of them singly, and the molecular motion is hence more easily assimilated, the same cause may explain the commencement of the phase of contraction on heating the alloy at a temperature 18° C. lower than that of the iodide of silver to which it owes this property.
7. If we compare one of the chlorobromiodides of silver, before described by the author (*Proc.* Roy. Soc., vol. xxv. p. 295) with the lead-silver iodide alloy, some curious anomalies present themselves. The alloy, AgI, AgBr, AgCl (lately also discovered as a mineral), contains 41·484 per cent. of iodide of silver, and 58·516 per cent. of the chloride and bromide of silver (which from an expansion point of view may be regarded as the same substance, because their co-efficients are practically the same). But although the mean coefficients of expansion of the chloride and bromide scarcely exceed those of the iodide of lead, and although the chlorobromiodide contains 8 per cent. more iodide of silver than the lead-silver iodide alloy, the amount of *contraction* by heat of the latter is more than twenty times greater than that of the former, although we must believe this effect to be solely due in each case to the presence of the iodide of silver.

Mathematical Society, April 14.—S. Roberts, F.R.S., president, in the chair.—The chairman briefly, but feelingly, alluded to the loss the Society had sustained by the recent death of Mr. T. Cotterill, M.A., formerly Fellow of St. John's College, Cambridge, who was for many years a member of the Council, and had always taken a warm interest in the Society.

—The following communications were made:—On the geodesic curvature of a curve on a surface, Prof. Cayley, F.R.S.—On operative symbols in the differential calculus, Prof. Crofton, F.R.S.—Note on the resolution in factors of numbers differing but slightly from π, Mr. J. W. L. Glaisher, F.R.S.—On the nature of the quadric represented by the general equation of the second degree in tetrahedral co-ordinates, and on the five focal quadrics of a cyclide, Mr. H. Hart, M.A.—The discrimination of the maximum or minimum path of a ray of light reflected at a given curve, Mr. H. M. Taylor, M.A.—On certain tetrahedra specially related to four spheres meeting in a point, and historical note on Dr. Graves's theorem on confocal conics, the President.

Physical Society, April 9.—Prof. W. G. Adams in the chair.—Prof. Helmholtz was elected an honorary member of the Society, and Dr. James Moser an ordinary member.—Dr. J. H. Gladstone read a note on thermal electrolysis, by himself and Mr. Alfred Tribe. The authors found that when sheet-silver was plunged into fused silver chloride or iodide of silver crystals of silver formed on the sheet. When copper was immersed in fused cuprous chloride copper crystals deposited on it, and when zinc was placed in melted zinc chloride or iron in melted ferrous chloride, these two metals crystallised on the plates. They found this to be due not to a difference in the physical condition of the rolled metals, but to the unequal heating of the different parts of the immersed metals. By the contact theory of voltaism there will be a difference of potential between the metal and the liquid chloride in contact with it, and this difference of potential will vary with temperature. Since all parts of the immersed metal cannot be supposed at the same temperature always, there is the possibility of a current being set up and consequent electrolysis of the salt. This view was corroborated by heating the metal unequally, when a crop of crystals appeared in the cooler part of the liquid. Again, two silver rods connected together were plunged, the one in a hotter, the other in a cooler part of fused silver chloride, and at the end of fifteen minutes the latter was studded with crystals of silver, whilst the former was clean. A galvanometer showed a stronger current between the rods, the greater the difference of temperature between the parts of the fluid in which they were placed, and transposing the rods reversed this current. These experiments bear on the nature of voltaic action, and form a lecture-illustration of the conversion of heat into electricity and chemical force. Mr. W. H. Walenn stated that he had found when zinc is immersed in an electro-brassing solution, crystals of brass (*i.e.*, zinc and copper) were deposited on it.—Capt. Abney exhibited a number of photographic negatives taken by himself and Col. Festing, by radiation through thin sheets of ebonite. The light from the positive pole of an electric lamp was sent through a thin sheet of ebonite $\frac{1}{64}$ inch thick, and photographs taken showed the radiation to have a low wave-length, from 8000 to 14,000. The carbon points of the lamp could be photographed through the sheet, and Col. Festing observed the sun's disk through it. The ebonite showed a grained structure, and different samples of ebonite gave different results, but all gave some result in course of time at least; old ebonite, like that used in some of Mr. Preece's experiments, scattering the light more than new ebonite.—Dr. Moser exhibited the passage of the rays through the ebonite to the audience by means of a galvanometer. Prof. Guthrie observed that Capt. Abney had proved that light as well as heat traversed the ebonite, and Dr. Coffin stated that the composition of ebonite apparently the same might vary considerably, and hence its radiant transparency might vary too.—Prof. Helmholtz addressed the meeting on the localisation of objects by the eyes. We estimate distance with one eye by the outlines of the more distant objects being covered by the nearer ones where they meet, and by the shadows thrown by the anterior objects. These conditions are very rarely overpowered by others, as, for instance, binocular vision. This is shown by Dove's pseudoscope and the fact that closing or blinding one eye makes little difference to the power of judging distance, especially when not very close to the eye. The relative shifting of objects, as the eye is moved from side to side, or to and fro, or up and down, which may be called the parallax of motion of the head, is also a strong factor in estimating distance. The author had concluded from a study of the stereoscope that the perception of the absolute convergence of our eyes is very indistinct, and that only differences of convergence related to apparently near or distant objects produce the stereoscopic effect. Recent observations of his show that the incongruity between the degree of convergence and the parallax of motion is perceived with great accuracy. Dr. Stone remarked that a person suddenly blinded in one eye acquires a new judgment of distance by moving the head (a habit seen in nocturnal birds), and in taking certain French stereoscopic pictures the camera is shifted to another point, so that the combined images produce an impression of smallness in the object. These facts corroborated Prof. Helmholtz's view; and Mr. Lewis Wright pointed out that santonin, which changes the sense of colour, also appears to change the sense of distance, perhaps by relaxing the muscular sense.

Geological Society, April 6.—J. W. Hulke, F.R.S., vice-president, in the chair.—Edward F. Boyd, Lieut. Herbert de Haga Haig, R.E.; J. C. Margetson, Edward David Price, and James Tonge were elected Fellows of the Society.—The following communications were read:—The microscopic characters of the vitreous rocks of Montana, U.S., by F. Rutley, F.G.S., with an appendix by James Eccles, F.G.S.—On the microscopic structure of devitrified rocks from Beddgelert, Snowdon, and Skomer Island, by F. Rutley, F.G.S.—The date of the last change of level in Lancashire, by T. Mellard Reade, C.E., F.G.S. The author described some observations made by him at Blundellsands, on the coast of Lancashire, near Liverpool, according to which, judging from the position of high-water mark, the land had gained considerably upon the sea between 1866 and 1874. The author adduced evidence in support of his view, and concluded that if the last change of level in South-West Lancashire was a downward one it could not have taken place within 2500 years.

Institution of Civil Engineers, April 5.—Mr. Brunlees, F.R.S.E., vice-president, in the chair.—The paper read was on the actual lateral pressure of earthwork, by Mr. B. Baker, M. Inst. C.E.

EDINBURGH

Royal Society, April 4.—Prof. Balfour in the chair.—Prof. Tait communicated the results of his experiments on the pressure errors of the *Challenger* thermometers, the correction for which, as originally furnished to the expedition, was $0^{\circ}\cdot5$ F. per mile of depth. The mode of experimenting was to subject the thermometers to considerable pressure in a hydraulic press, which was essentially a strong steel cylinder that was warranted to stand a pressure of 25 tons weight on the square inch. It was supported in an upright position upon a strong tripod stand. Water was filled in from above; and into the upper end of the cylinder there was lowered a tight-fitting plug which was fixed in position by a transverse steel bolt. The lower end of the cylinder was connected through a narrow copper tube to a hydraulic pump, which, by pumping in water to the cylinder, raised the pressure to the required amount. At three tons pressure an average effect of $1^{\circ}\cdot5$ F. was produced upon the inclosed thermometers. Before drawing any conclusions as to the correction to be applied in deep-sea sounding, it was necessary to consider how far this effect could be explained as resulting from the peculiar conditions under which the experiments were made. From the known compressibility of glass it was calculated that the volume of the bore of a thermometer tube, closed at both ends, would be diminished by only one-thousandth part for an increase of pressure of one ton weight on the square inch; and from a direct experiment made with a metre-long tube this was proved to represent very approximately the real effect. Hence it was quite out of the question that this could have any appreciable effect on such comparatively short thermometers as those of the *Challenger*, which were besides subject to much graver errors, such as those arising from the shifting of the indices during the ascent from the depths, or even from the effect of parallax when taking the reading. The direct action of pressure may then be disregarded, and the effect produced upon the thermometers in the compression apparatus must be due to secondary effects of pressure, such as evolution of heat. The various sources of heat were four: 1. Heating of the water by compression. This depends greatly on the original temperature of the water, being *nil* at the point of maximum density (40° F.), and larger for higher temperatures. One-fourth of the total effect is due to this. 2. Heating of the water due to pumping in through the narrow tube. This accounts for three-twentieths of the effect. 3. Heating of the vulcanite frame by compression. This explains another fifth. 4. Heating due to the effect upon the protecting bulb. This probably explains the remaining two-fifths of the effect. In this last case however there is not only compression, but distortion; and of the thermal effects of such a strain no one yet knows anything. These four sources of error

cannot be supposed to exist under the conditions in which deep-sea temperatures are taken; and the only other possible source, that namely due to the direct effect of pressure, gives rise to an error which requires a correction of only 0°·04 F. per mile of depth. In the course of the description of experiments Prof. Tait had occasion to describe the various kinds of pressure-gauges which he had found it necessary to devise, the ordinary forms of gauge being altogether useless for scientific work.—Mr. W. W. J. Nicol read a paper on the action of heat on thioformanilide, being an account of experiments he had made in Prof. Hoffman's laboratory at Berlin during the preceding winter.—Mr. Patrick Geddes read the second instalment of his scheme for the classification of statistics. In it he discussed the arrangement of statistics relating directly to the organisms of the society. Three great parallel classes, A, B, C, were formed: A being concerned with the *source* of the organisms forming a community as arising from survival, immigration, and birth; C with the *loss*, from emigration and death; while B contained the biological and social characteristics of the individuals forming a community at any given instant of time. Classes A and B formed the one side and C the other side of the social balance sheet. In treating of occupations the same three classes appeared again: A dealing with operations on matter and energy, B with services rendered to society (including education, government, &c.), and C forming the class of the essentially unproductive, *e.g.* the unemployed, the disabled, the destructive, &c. The question of partition, both mediate and ultimate, amongst the organisms of matter and energy fell next for discussion; and this led on to the final classification of uses made after partition, in all of which it was shown that the classification fitted naturally into the three original classes, A, B, and C, indicated above. In a future paper Mr. Geddes hoped to demonstrate the *practical* value of his system.

Vienna

Imperial Academy of Sciences, April 7.—L. Fitzinger in the chair.—The following papers were read:—Dr. G. Becka, on the orbit of the "Ino" planet (No. 173).—Dr. E. Ludwig, on a new method for the quantitative determination of uric acid.—Dr. D. Dublier, on the influence of continual use of carbonate of soda on the composition of the blood.—Dr. James Moser, electrostatic investigations especially into the ramification of induction on the differential inductometer and electrophorus.—Dr. Moritz Holl, on the blood-vessels of the placenta of man.—L. Haitinger, on nitro-olefines.

Imperial Institute of Geology, March 15.—E. Kittl, on a recent find of Listriodon (found at Nussdorf, near Vienna, in 1879).—Dr. E. Mojssisowics, on the cephalopod-fauna of the Triassic formations at Mora d'Ebro, in Spain.—K. M. Paul, on the occurrence of ozokerite and petroleum at Boryslaw (Gallicia).

April 5.—E. Kittl, on Bohemian spas.—Baron H. Fullon, observations on crystallisation.—Dr. V. Hilber, on the terminal stratifications of gypsum in Eastern Gallicia.

Paris

Academy of Sciences, April 11.—M. Wurtz in the chair.—The following papers were read:—On peroxide of ethyl, by M. Berthelot. This may be prepared by sending through anhydrous ether, for several hours, a slow current of quite dry and strongly ozonised oxygen. The formation of oxygenated water by action of ozone on ether is not immediate, but by destruction of a first compound, viz. peroxide of ethyl. This substance is a sesquioxide $C_{16}H_{20}O_6$.—On the Eulerian integral of the second species, by M. Gyldén.—Researches on the liquefaction of gaseous mixtures, by MM. Cailletet and Hautefeuille. Operating with a gas easily liquefiable and a so-called permanent gas, in capillary tubes, total liquefaction (yielding a homogeneous liquid) is obtained by first compressing the mixture at a temperature so high that the strongest pressures prove powerless to abolish the gaseous state, then lowering the temperature regularly, so that all points of the tube pass at the same time through the temperature at which is produced a change of state. The authors thus obtained condensed carbonic acid, holding a large proportion of oxygen, hydrogen, or nitrogen, these latter substances concurring to form the liquid, though the temperature was too high for them to exist separately in that state. The results of experiment with cyanogen and carbonic acid are analysed. The assimilation (generally very imperfect) of solution of a gas to its liquefaction probably here applies. The mixture retains its characters at temperatures considerably above that corresponding to the critical point of its less easily liquefied element.—On the lines of iron in the sun, by Mr. N. Lockyer. He shows reason for believing that iron does not exist in the heart of the sun, but only its constituents, and these exist at different levels in the sun's atmosphere and produce more complex forms by condensation.—On pucerons attacked by a champignon, by MM. Cornu and Brongniart. The insect belongs to the cycle of development of *Tetraneura rubra*, which produces the red galls of elm. The fungus is a *Pleospora*; it attacks the dead puceron. It is probably incapable of affecting much the multiplication of phylloxera.—On the integration of linear equations by means of Abelian functions, by M. Poincaré.—On formulæ of representation of functions, by M. du Bois-Reymond.—Study of the vapour of bisulphydrate of ammonia, by M. Isambert. The substance is less volatile in presence of its elements than *in vacuo*, or in an inert gas such as hydrogen.—On chlorides, bromides, and iodides of sulphur, by M. Ogier. A thermo-chemical study.—On the development of *Tricuspidaria nodulosa* or *Triænophorus nodulosus* of Rudolph, and on its cysticercus, by M. Megnin. The perches of the Seine are greatly affected by this parasite at present.—Studies on some points of the anatomy of *Sternaspis scutata*, by M. Rietsch.—On the different species of bears whose remains are buried in the cavern of Lherm (Ariège), by M. Filhol. Remains of an enormous *Ursus arctos* (apparently) have been found among about 100 bones of *Ursus spelæus*. M. Filhol doubts the descent of the former bear from the latter. He supposes that *Ursus arctos*, appearing in distant regions (perhaps North America), gradually advanced and was substituted in our countries for *Ursus spelæus*. Bone fragments of a new type of bear have been found in this cave. The author names it *Ursus Gaudryi*. The fossil femur of an enormous lion has also been found.—Production of a hydrated silicate of baryta in crystals, by M. Le Chatelier. This appears on the inner surface of vessels of baryta water left standing uncleaned a long time.—On the production of a crystalline phosphide of iron and of anorthite felspar in the fires of the Commentry coal-pits, by M. Mallard.—On the swelling of the Seine during the winter of 1881, by M. Lemoine. The Seine at Paris has been pretty high from the middle of January to the middle of March. Usually (as M. Belgrand has shown) the maximum of flood at Paris is due to the waters of small torrential rivers mostly in the upper part of the valley and issuing from impermeable strata. But last winter, it is chiefly the rivers nearest Paris, those of Brie, that, by their quite unusual swelling, have brought on the maximum (which has therefore come with great rapidity). The subsoil of La Brie is like a sponge, and when it is gorged with water the least rain causes important floods.

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